THE

LONDON, EDINBURGH, and DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

VOL. XII.—FOURTH SERIES.

JULY—DECEMBER, 1856.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

*J. B. Pinelli ad Mazonium.*

# CONTENTS OF VOL. XII.

## (FOURTH SERIES.)

## NUMBER LXXVIII.—SEPTEMBER.

## NUMBER LXXIX.—OCTOBER.

## NUMBER LXXX.—NOVEMBER.

## NUMBER LXXXI.—DECEMBER.

## NUMBER LXXXII.—SUPPLEMENT TO VOL. XII.

### ERRATA IN VOL XI.

Page 157, line  5, *for* half the side *read* the side.
—  159,  — 24, *for*  2,000,000  *read*  $5200 \times 10^6$;  and *for*  8,200,000
*read*  $21000 \times 10^6$.
—  *ib.*  — 30, *for* 10 tons *read* $53,600 \times 10^6$ tons ; and *for* 42 tons !
*read* $874,000 \times 10^6$ tons !

### ERRATA IN VOL XII.

Page 394, line 19, *for* property *read* quality.
—  395,  — 7 from bottom, *for* a given result *read* given a result.
—  396,  — 14 from bottom, *for* that *read* steel.

ALERE FLAMMAM.

I. *On the Demonstration of* Fresnel's *Formulas for Reflected and
Refracted Light; and their Applications.* *By the* Rev. BADEN
POWELL, *M.A., F.R.S. &c., Savilian Professor of Geometry in
the University of Oxford*[*].

1.   A QUESTION between two fundamentally different views
of the theory of polarization, which has been long
agitated among inquirers into the undulatory theory, viz. as to
the direction of the plane of *vibrations* in relation to that of
*polarization,* has of late excited more peculiar interest, partly
from the announcement, a few years ago, of a remarkable *crucial*
experiment by Professor Stokes, and partly from several subse-
quent investigations, especially the recent elaborate discussion of
the general bearing of the experimental evidence by M. Haidinger.

The revival of this question recalls the attention of the student
to the very unsatisfactory condition in which the elementary de-
monstration of those parts of the theory on which it depends has
long been left, and from which recent speculations have done
little to deliver it.

2. The well-known and remarkable formulas originally given
by Fresnel to express the amplitudes of the vibrations, and thence
the intensities, of reflected and refracted rays of polarized light
(for singly-refracting media), which are found to represent so
beautifully all the observed changes,—in fact including the whole
doctrine of plane polarization, and thus invaluable as inductive
laws,—yet long remained confessedly defective as to their system-
atic deduction from theory.

3. Fresnel, indeed, with that marvellous sagacity for which
he was so conspicuous, satisfied himself of their truth by reason-

ing on assumptions in some instances confessedly empirical, in others dependent on analogies, or hypotheses not free from doubt, and at any rate little connected into a system.

4. This investigation, whose questionable points are so fairly stated, and ably grappled with, by Mr. Airy in his tract on the Undulatory Theory (1831, art. 128 *et seq.*), has been since pursued on different principles by M. Cauchy, and especially by the late Prof. Maccullagh in his memoir "On the Laws of Crystalline Reflexion and Refraction" (Mem. Roy. Irish Acad. vol. xviii. 1838), whose views have been ably but briefly expounded by Dr. Lloyd in his Lectures on the Wave Theory (part 2. p. 30, 1841). More recently, Mr. Power has investigated the subject by a systematic analysis, directed to other objects, but including an important element in these deductions ("On Absorption of Rays," &c., Phil. Trans. 1854, part 1).

5. But among these distinguished philosophers there exists considerable diversity, and even contradiction of views. Nor, so far as I am aware, has the subject been so discussed as to enable us to trace the source of these discrepancies, or fairly to estimate the claims of the opposing theories, or the force of the experimental results which bear upon them. Thus it seems highly desirable, that questions affecting so fundamental a part of the undulatory theory should be cleared up and placed on an unassailable basis.

Having long ago thrown aside some investigations on the subject, in which I was then engaged, I have of late had my attention recalled to the question, and have thus been induced to revise and extend those investigations, in the hope of contributing towards the settlement of the points involved, or at any rate of putting the whole discussion before the student in a connected point of view; with which object I have been led to commence *ab initio*, so that those who have only an elementary acquaintance with the theory may be enabled to follow the deductions without difficulty, and may here be furnished with that systematic elucidation which is not, as far as I am aware, to be found in any existing publication.

### Theoretical Views.

6. The formulas for the amplitudes of the incident, reflected, and refracted rays, as given by Prof. Maccullagh and later writers, though closely corresponding with those of Fresnel, and fulfilling generally the same conditions, yet differ from them in certain cases as to the *sign*, and in others as to the *values* of the expressions.

7. But the main point of difference and difficulty consists in this: Fresnel investigates two sets of formulas; one set (H) for

the respective rays deduced on the supposition that the vibrations are *perpendicular* to the plane of incidence, another set (K) on the supposition that they are *parallel* to that plane. Now those of Maccullagh, which correspond closely to Fresnel's first set (H), are deduced on the contrary supposition of vibrations *parallel* to incidence, while those corresponding to Fresnel's second set (K) are for vibrations *perpendicular* to that plane.

8. In either investigation the formulas (K) are those which represent evanescence of the light at the polarizing angle, while the formulas (H) represent brightness at that incidence.

But when a ray vanishes at the polarizing angle, we know that its plane, of this second incidence, must be *perpendicular* to that of its first incidence or original polarization. Hence, according as the vibrations (K) may be *parallel* or *perpendicular* to this second plane of incidence, they must be respectively *perpendicular* or *parallel* to the first or plane of polarization. The question thus reduces itself to whether, in polarized light in general, the *vibrations* are *parallel* or *perpendicular* to the plane of *polarization*.

9. M. Cauchy, in an earlier paper (*Mem. Instit.* vol. x. p. 304), had inferred with Maccullagh, from dynamical views, that the vibrations are *parallel* to the plane of polarization. But in a later memoir (*Bull. Math.* July 1830) he deduces formulas corresponding to Fresnel's on the hypothesis of vibrations *perpendicular* to the plane of polarization, and even more formally renounces his earlier opinion and returns to that of Fresnel. He also connects similar equations with higher dynamical principles in the *Nouv. Exercices Math.* (liv. 7).

### Synopsis of Formulas referred to.

10. Fresnel's formulas for vibrations *perpendicular* to the plane of incidence ($h$ being the amplitude of the incident, $h'$ of the reflected, and $h_i$ of the refracted rays, and dividing by $h$, $i$ being the angle of incidence, $r$ of refraction), are—

$$h' = \frac{-\sin(i-r)}{\sin(i+r)}, \quad h_i = \frac{2\sin r \cos i}{\sin(i+r)}; \quad \cdot \quad \cdot \quad \cdot \quad \textbf{(H)}$$

and for vibrations *parallel* to the plane of incidence (similarly designated by $k$, $k'$, and $k_i$),

$$k' = \frac{\tan(i-r)}{\tan(i+r)}, \quad k_i = \left(1 - \frac{\tan(i-r)}{\tan(i+r)}\right)\frac{\cos i}{\cos r}. \quad \cdot \quad \cdot \quad \textbf{(K)}$$

These last may be otherwise expressed thus :

$$k' = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r},$$

$$k_i = \left(\frac{2\sin 2r}{\sin 2i + \sin 2r}\right)\frac{\cos i}{\cos r} = \frac{4\sin r \cos i}{\sin 2i + \sin 2r}.$$

11. Here we may observe, in the numerator of $h_{\prime}$,

$$2 \sin r \cos i = \frac{1}{\mu} \sin 2i \, ;$$

and of $k_{\prime}$,

$$2 \sin 2r \, \frac{\cos i}{\cos r} = \frac{1}{\mu} 2 \sin 2i.$$

12. It is also desirable to notice, that these expressions are the same as those given in Mr. Airy's Tract, § 129, under the slightly different form in which they directly result from the peculiar process there pursued, viz. writing $\sin (r - i)$ and $- \tan (r-i)$.

Also the numerator of $k'$ is *positive* for all values of $(i-r)$, which is necessarily less than 90°, while the denominator becomes $\infty$ at $(i+r) = 90°$, which, according to Brewster's law, is the polarizing angle, and for greater values continues *negative*.

13. Prof. Maccullagh's formulas are,—
for vibrations *parallel* to the plane of incidence,

$$h' = \frac{\sin (i-r)}{\sin (i+r)}, \quad h_{\prime} = \frac{\sin 2i}{\sin (i+r)} ; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(H')}$$

for vibrations *perpendicular* to the plane of incidence,

$$k' = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r} = \frac{\tan (i-r)}{\tan (i+r)},$$

$$k_{\prime} = \frac{2 \sin 2i}{\sin 2i + \sin 2r} = 1 + \frac{\tan (i-r)}{\tan (i+r)}. \quad \cdot \quad \cdot \quad \text{(K')}$$

These last are sometimes expressed under the forms

$$k' = \frac{\sin (i-r) \cos (i+r)}{\cos (i-r) \sin (i+r)}, \quad k_{\prime} = \frac{\sin 2i}{\cos (i-r) \sin (i+r)}.$$

14. Comparing these formulas with Fresnel's (distinguished by using roman letters), we may observe from (11),

$$h' = -\mathrm{h}', \quad h_{\prime} = \mathrm{h}_{\prime}\mu,$$
$$k' = \mathrm{k}', \quad k_{\prime} = \mathrm{k}_{\prime}\mu.$$

Here also $k_{\prime}$ undergoes the same change of sign at the incidence of complete polarization.

### *Densities and Vibrating Masses.*

15. In deducing these formulas, it is in all cases necessary to express the ratio of the *masses* of æther simultaneously vibrating without and within the medium; and the differences in the respective formulas are mainly dependent on the very opposite suppositions made by the several philosophers as to the *density* of the æther in different media,—Fresnel supposing it *more* dense

within the denser medium; Cauchy, *less* dense; and Maccullagh, *equally* dense in all media. The last-named writer has argued that refraction cannot be dependent on the *density* of the æther as such. He especially observes, that "in doubly-refracting crystals, the density, being independent of the direction, could not be conceived to vary with the refractive index" (p. 39). And Prof. Stokes has observed, that in the vibrations of æther, "diminution of velocity seems capable of being accounted for on several distinct hypotheses."

16. The expressions for the masses of æther vibrating in the same time without and within the denser medium, are obtained on these different suppositions as to the density of the æther, as follows :—

If $v$ be the velocity of the incident ray, $v_i$ that of the refracted, and the index $\mu = \dfrac{v}{v_i}$, then at a *perpendicular incidence*, the simultaneously vibrating masses will be simply

$$\frac{(m)}{(m_i)} = \frac{v}{v_i} = \mu = \frac{\sin i}{\sin r}.$$

If the densities be $\delta$, $\delta_i$, then, according to the view of Fresnel, $\delta_i > \delta$, and

$$\frac{\delta}{\delta_i} = \frac{\sin^2 r}{\sin^2 i} = \frac{1}{\mu^2},$$

and we must multiply in this ratio, which gives

$$\frac{(m)}{(m_i)} = \frac{1}{\mu}.$$

If $\delta = \delta_i$, according to the view of Maccullagh,

$$\frac{(m)}{(m_i)} = \mu.$$

17. In either case, for *oblique incidences* we must multiply by the rectangular breadth of the rays on the same base or section of the surface, which will be as $\cos i : \cos r$, or

$$\frac{m}{m_i} = \frac{(m) \cos i}{(m_i) \cos r}.$$

Thus, according to Fresnel,

$$\frac{m}{m_i} = \frac{1}{\mu} \frac{\cos i}{\cos r} = \frac{\sin r \cos i}{\sin i \cos r}.$$

According to Maccullagh,

$$\frac{m}{m_i} = \mu \frac{\cos i}{\cos r} = \frac{\sin 2i}{\sin 2r}.$$

18. It may here be observed, that if we admit equal densities, we must nevertheless suppose some retarding power in the æther within the denser medium. It is still conceivable that this may follow the *same law* as that of increased density, and that thus Fresnel's formula might still apply.

Or again, this reduces itself to the condition, that for perpendicular incidence we should have

$$\frac{(m)}{(m_{\prime})} = \frac{1}{\mu},$$

which might be simply the original condition, without involving the division by $\mu^2$, as above, and might be dependent directly on some hypothesis assumed as to the constitution of the æther.

19. Expressing Fresnel's values by roman letters, and comparing with Maccullagh's, we have

$$\frac{\mathrm{m}}{\mathrm{m}_{\prime}} = \frac{m}{m_{\prime}} \frac{1}{\mu^2}.$$

20. According to the view of Cauchy, the density is *diminished* in the denser medium. If we suppose it diminished, according to the same law,

$$\frac{\delta}{\delta_{\prime}} = \mu^2 \ \text{ and } \ \frac{m}{m_{\prime}} = \mu^3 \frac{\cos i}{\cos r}.$$

21. Mr. Power, taking $\alpha$ for the distances of the molecules without, and $\alpha_{\prime}$ within, the medium, obtains what is equivalent to

$$\frac{m}{m_{\prime}} = \frac{\alpha_{\prime}^3 v \cos i}{\alpha^3 v_{\prime} \cos r} = \frac{\delta^3 v \cos i}{\delta_{\prime}^3 v_{\prime} \cos r};$$

but having avoided any assumption of the law of refraction at the outset, he deduces (§§ 18, 28) the value

$$\mu = \frac{\alpha_{\prime}^3 v}{\alpha^3 v_{\prime}} = \frac{\delta^3 v}{\delta_{\prime}^3 v_{\prime}},$$

which seems irreconcileable with the admitted principle $\mu = \dfrac{v}{v_{\prime}}$, unless by supposing $\delta = \delta_{\prime}$, which would agree with Maccullagh's view. Or if we could have $\dfrac{\delta^3}{\delta_{\prime}^3} = \dfrac{1}{\mu^2}$, the expression would agree with Fresnel's view; or if $\dfrac{\delta^3}{\delta_{\prime}^3} = \mu^2$, with that of diminished density. But as neither of these suppositions seem reconcileable with admitted principles, it will not be material to discuss them further.

### Equivalent Vibrations.

22. As to the general nature of the vibratory forces concerned, it will be on all hands admitted that the vibratory force of the

external æther or incident ray is the sole exciting cause of that communicated, partly to the reflected, partly to the refracted ray; so that the vibratory force of the incident waves must be distributed between the reflected and refracted.

23. An obvious geometrical relation is derived from the known *directions* of the incident, reflected, and refracted rays, which, with the parallel to one of them, form a triangle, whose angles being known, the sides are in the ratios of their *sines* : and the same relation subsists between the portions of the amplitudes at right angles to the rays, and supposed to lie in the same plane.

24. The triangle formed by the directions of the incident, reflected, and refracted rays, will have—
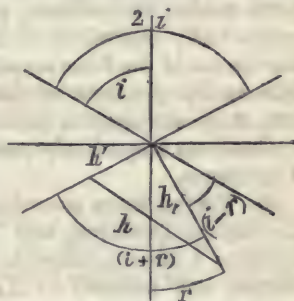


The angle formed by the incident and reflected rays $= 2i$

...     ...     incident and refracted rays $= (i-r)$

...     ...     reflected and refracted rays $= (i+r)$.

Then the sides, or parts of the rays or amplitudes intercepted will be

$$\frac{h'}{h} = \frac{\sin(i-r)}{\sin(i+r)}, \quad \frac{h_{,}}{h} = \frac{\sin 2i}{\sin(i+r)}. \quad \cdot \quad \cdot \quad \cdot \quad (\mathrm{H}')$$

25. Again, these sides have mechanically the relation of the resultant and components of the vibratory motions in their respective directions. Hence this simple relation is adopted by Prof. Maccullagh to express the relative amplitudes or velocities : and this is the more remarkable, since (as he observes) they so nearly resemble the expressions adopted by Fresnel, on the opposite hypothesis of vibrations *perpendicular* to the plane of incidence, with which this construction can have no relation.

26. For vibrations *perpendicular* to the plane of incidence, or when the vibrations of all the three rays are *parallel to the surface* of the medium, it is also inferred that the amplitudes must be mechanically equivalent, or, as more distinctly argued by Mr. Power, that " a particle at the surface of separation will be at one and the same moment performing its phase to the incident, to the reflected, and to the refracted rays, with transverse velocities proportional to the amplitudes of those rays respectively ;" and that " since this particle cannot move in more than one way at once, it is clear that the two latter must be equivalent to the former, according to the law of the composition of velocities."

**27.** But these are merely particular cases of the general theory of "equivalence of vibrations," first, I believe, systematically proposed by Prof. Maccullagh as the basis of his higher investigation of the laws of reflexion and refraction at the surfaces of crystals. Yet some cases of it appear to have been assumed by Fresnel, though under a slight, but material, difference of view. The general principle common to both is, that in "two contiguous media, the incident, reflected, and refracted vibrations are mechanically equivalent:" but a difference in conception of the *distribution* of the force among them gives rise to a corresponding difference in the form of the expressions; on either view, however, these expressions indicate conditions *distinctively* applying to vibrations respectively parallel and perpendicular to the plane of incidence.

**28.** Taking three co-ordinate planes, XY that of the surface, XZ that of incidence, and YZ perpendicular to incidence, any vibration $h$ passing through the origin taken at the point of incidence, and inclined to XZ by an angle $\theta$, and to XY by $\phi$, may be resolved into

$$y = h \sin \theta, \quad x = h \cos \theta \cos \phi, \quad z = h \cos \theta \sin \phi.$$

Then for $h$ of the incident ray we have $\phi = i$ and $\theta$

... $h'$ of the reflected ray we have $\phi = i$ and $\theta'$

... $h_{,}$ of the refracted ray we have $\phi = r$ and $\theta_{,}$.

The law of equivalent vibrations, according to Prof. Maccullagh, is expressed by these relations between the resolved parts respectively:—

in $z$, $h \cos \theta \sin i + h' \cos \theta' \sin i = h_{,} \cos \theta_{,} \sin r$;

in $y$, $h \sin \theta + h' \sin \theta' = h_{,} \sin \theta_{,}$;

in $x$, $h \cos \theta \cos i + h' \cos \theta' \cos i = h_{,} \cos \theta_{,} \cos r$.

**29.** Hence if the plane of vibration coincide with YZ perpendicular to incidence, $\theta = \theta' = \theta_{,} = 90°$, and the law becomes simply

$$h + h' = h_{,}.$$

If it coincide with XZ, $\theta = \theta' = \theta_{,} = 0$, and it becomes

$$h \cos i + h' \cos i = h_{,} \cos r,$$

or

$$h + h' = h_{,} \frac{\cos r}{\cos i} = (h_{,}).$$

These two may be included in the formula

$$(h_{,}) = h_{,} \left( \sin \theta + \frac{\cos r}{\cos i} \cos \theta \right). \quad . \quad . \quad . \quad \text{(L)}$$

$\theta = 90°$ gives $(h_{,}) = h_{,}$, and $\theta = 0$ gives $(h_{,}) = h_{,} \dfrac{\cos r}{\cos i}$.

Such is the principle of equivalent vibrations, according to Prof. Maccullagh ; in other words, it is founded on the proposition that " *the incident and reflected vibrations are equivalent to the refracted.*"

30. It does not appear to what extent Fresnel had adopted any *generalized* view of this kind ; but in the deduction of his formula, he introduces an equation which is equivalent to

$$h - h' = \frac{\cos r}{\cos i} h_{\prime}$$

for vibrations *parallel* to the plane of incidence ; and if we suppose this to be the expression for the law of equivalence, in that case we must also take for that of *perpendicular* vibrations,

$$h - h' = h_{\prime}.$$

In other words, the law of equivalent vibration, according to Fresnel's view, will differ from that of Maccullagh in that it affirms the proposition that " *the refracted and reflected vibrations are equivalent to the incident.*"

These values of $h_{\prime}$, which we will call (L′), may be combined in the same expression as before (L).

### *Equation of* vis viva.

31. That the *amplitudes* of the vibrations are the measures of the *velocities* of those vibrations whose time is constant, is obvious; and further, that the *square* of the amplitude or velocity, *multiplied* by the *vibrating mass*, is the true measure of the *intensity* ; and consequently that the principle of *vis viva*, of which it is a simple application, is true, undoubtedly receives ample proof *à posteriori*, inasmuch as all the calculations founded on this principle agree to such extreme accuracy with the experimental results, whatever question may have existed as to its establishment *à priori*.

32. Mr. Power, assuming the general principle, but following an original analytical method, deduces directly expressions for the *vis viva* of the incident, reflected, and refracted ray respectively ; and applies them by introducing the same expressions for the amplitudes as those of Maccullagh, and on the same supposition as to the directions of the planes of vibration, though not on the same hypothesis as to the constitution of the æther, with respect to which he is led to conclusions of a peculiar kind, and which have been considered by some as questionable, but the consideration of which is foreign to the present inquiry.

33. Assuming the ordinary formula for vibrations in *any plane*,

$$u = h \sin \frac{2\pi}{\lambda} (vt - x) ;$$

if $n$ be the number of vibrations in a unit of time, $v = n\lambda$; and for two media,

$$\frac{v}{v_{\prime}} = \mu, \quad \frac{v}{\lambda} = \frac{v_{\prime}}{\lambda_{\prime}} = n \text{ for homogeneous light.}$$

34. If we consider, first, a single line of vibrating molecules, we may investigate the *vis viva* of that *line*; but from this proceeding to the vibrating *mass*, we must multiply the previous expression by the density (or equivalent retarding property), which we may express generally by $\delta$ and $\delta_{\prime}$ for the external and internal æther; and by the rectangular breadths of the oblique rays on the same base or section of the surface, which will be respectively proportional to $\cos i$ and $\cos r$: thus we shall have in general for the multipliers, $\delta \cos i$ and $\delta_{\prime} \cos r$; the former for the incident and reflected, the latter for the refracted ray.

35. Mr. Power's investigation is restricted to a particular hypothesis as to the density, but may be more simply and generally followed out thus:—

For a length $dx$, in which the molecules have a common velocity, we may take for that velocity,

$$\mathbf{v} = \frac{du}{dt} = \frac{2\pi h v}{\lambda} \cos \frac{2\pi}{\lambda}(vt - x);$$

and for a single *line* of vibrating molecules, the *vis viva*

$$\int \mathbf{v}^2 dx = \frac{4\pi^2 h^2 v^2}{\lambda^2} \int \cos^2 \frac{2\pi}{\lambda}(vt - x)dx.$$

For a portion from $x$ to $x + \lambda$, the integral is easily found to reduce to $\frac{\lambda}{2}$; and thus for those limits,

$$\int_{x+\lambda}^{x} \mathbf{v}^2 dx = \frac{2\pi^2 h^2 v^2}{\lambda} = 2\pi^2 n h^2 v = (p);$$

and for the vibrating *mass*, as before explained (34),

For the incident ray $p = 2\pi^2 n h^2 v \delta \cos i$

...   reflected ray $p^{\prime} = 2\pi^2 n h^{\prime 2} v \delta \cos i$

...   refracted ray $p_{\prime} = 2\pi^2 n h_{\prime}^2 v_{\prime} \delta_{\prime} \cos r$.

But since the principle of *vis viva* gives $p = p^{\prime} + p_{\prime}$, we have the equation connecting the *vires vivæ*,

$$\mu\delta \cos i(h^2 - h^{\prime 2}) = \delta_{\prime} \cos r h_{\prime}^2,$$

or generally

$$m(h^2 - h^{\prime 2}) = m_{\prime} h_{\prime}^2 ; \quad \cdots \quad \cdots \quad \text{(M)}$$

where, on substituting the values of $m$ and $m_{\prime}$ on the respective hypotheses, we can express the *vires vivæ* equally on the respective views of Fresnel, Maccullagh, and others.

36. In some of these investigations, reference has been made to the mechanical doctrine of the impact of elastic bodies, to which the communication of vibratory motion in æther presents so striking an analogy. The well-known equations expressing the law of impact are adopted directly as the basis of Fresnel's proof in the case of vibrations perpendicular to the plane of incidence, giving rise to his formulas (H).

The very same equation is deduced by Maccullagh from his abstract dynamical principles, without reference to the above-mentioned analogy; but applied on the same hypothesis as to the vibrations, and on his assumption as to the density, it produces his formulas (K).

37. This deduction, which is independent of any particular supposition as to densities or direction of vibrations, is as follows:—

Combining equations (L) and (M), we have

$$\frac{h^2 - h'^2}{h_{,}^2} = \frac{m_{,}}{m} = \frac{(h + h')(h - h')}{(h + h')^2} = \frac{h - h'}{h + h'};$$

whence,

$$\frac{m - m_{,}}{m + m_{,}} = \frac{h'}{h}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(N)}$$

whence again,

$$(h + h_{,}) = 2m = h_{,}, \text{ or } \frac{h_{,}}{h} = \frac{2m}{m + m_{,}}. \quad . \quad . \quad . \quad \text{(O)}$$

### Deduction of Expressions for the Amplitudes.

38. (I.) On the hypothesis of *equal* densities, we have directly, on substituting in the equation (N) the values of the masses and dividing by $k$, supposed $= 1$,

$$m = \sin 2i, \quad m_{,} = \sin 2r,$$

$$k' = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r}, \quad k_{,} = \frac{2 \sin 2i}{\sin 2i + \sin 2r}. \quad . \quad . \quad \text{(K')}$$

These are Maccullagh's formulas deduced in the same way, and here (as before) $k'$ changes sign at $i + r = 90°$.

39. It is also evident that these values fulfil the equations of *vis viva* (M), viz.

$$(k^2 - k'^2) = k_{,}^2 \frac{\cos r}{\mu \cos i} = \sin 2i \sin 2r,$$

as well as that of equivalence of vibrations (L),

$$k + k' = k_{,} = 2 \sin 2i;$$

which last shows that they belong necessarily to vibrations *perpendicular* to the plane of incidence.

40. For vibrations *parallel* to the plane of incidence :—since we

are here concerned only with the *ratios* of the amplitudes, we may suppose one of them the same as in the last case, and thence find the others, which will be different in order to fulfil the different conditions. Thus assuming $k_{\prime}=h_{\prime\prime}$, the condition of equivalence (L) gives

$$(h+h')=h_{\prime}\frac{\cos r}{\cos i}=2 \sin 2i\,\frac{\cos r}{\cos i}=4 \sin i \cos r\,;$$

and from this, with that of *vis viva* (M), viz.

$$(h^2-h'^2)=h_{\prime}^2\,\frac{\cos r}{\mu \cos i}=16 \sin i \cos i \sin r \cos r=(h+h')(h-h')$$

$$(h-h')=4 \cos i \sin r$$

$$2h=4 \sin i \cos r+4 \cos i \sin r$$

$$2h'=4 \sin i \cos r-4 \cos i \sin r\,;$$

and since always $\sin i > \sin r$ and $\cos r > \cos i$, the last value is always positive. Hence dividing by $h$, considered $=1$,

$$h'=\frac{\sin (i-r)}{\sin (i+r)}, \quad h_{\prime}=\frac{\sin 2i}{\sin (i+r)}\,; \quad . \quad . \quad . \quad \text{(H)}$$

the same values as those derived from the geometrical construction before mentioned (24).

41. (II.) On the hypothesis of *increased* density (or of increased retardation supposed to be represented by the same law), we have for the masses $m=\sin r \cos i$, $m_{\prime}=\sin i \cos r$, in which, since $\sin r < \sin i$ and $\cos i < \cos r$, we have always $m < m_{\prime}$, and therefore when we substitute in equation (N), we have

$$m-m_{\prime}= -(\sin i \cos r- \sin r \cos i)= - \sin (i-r).$$

Thus from that equation we have directly, dividing by $h$, as before,

$$h'=\frac{-\sin (i-r)}{\sin (i+r)}, \quad h_{\prime}=\frac{2 \sin r \cos i}{\sin (i+r)}. \quad . \quad . \quad \text{(H)}$$

These are Fresnel's formulas as derived originally from assuming the same equation (N) on the analogy of impact.

42. But it is also evident from the process of elimination by which that equation is here obtained, that these values fulfil the equation of *vis viva*,

$$(h^2-h'^2)=h_{\prime}^2\mu\frac{\cos r}{\cos i} = \sin 2i \sin 2r,$$

as well as that of equivalence of vibrations (L),

$$h+h'=h_{\prime}, \text{ or } \sin (i+r)-\sin (i-r)=2 \sin r \cos i\,;$$

which last proves that they *necessarily* belong to vibrations *perpendicular* to the plane of incidence.

43. For vibrations *parallel* to the plane of incidence:—as

before, if we assume $k_{\prime}=2h_{\prime}$ to find $k$ and $k'$, the condition of equivalent vibrations (L) gives

$$k+k' = k_{\prime}\frac{\cos r}{\cos i} = 4 \sin r \cos i \frac{\cos r}{\cos i} = 2 \sin 2r.$$

Also the equation of *vis viva* (M) becomes

$$(k^2 - k'^2) = k_{\prime}^2\mu\,\frac{\cos r}{\cos i} = 4 \sin 2i \sin 2r = (k+k')(k-k'),$$

whence

$$k - k' = 2 \sin 2\,i$$
$$2k = \sin 2r + \sin 2i$$
$$2k' = \sin 2r - \sin 2i.$$

Hence, as before,

$$k' = \frac{\sin 2r - \sin 2i}{\sin 2r + \sin 2i}, \quad k_{\prime} = \frac{4 \sin r \cos i}{\sin 2r + \sin 2i}. \quad \cdot \quad \cdot \quad \text{(K)}$$

And since for $i=\varpi$ or $(i+r)=90°$, $\sin 2r = \sin 2i$, we have $k'=0$; for incidences, $i < \varpi$, we have $\sin 2r < \sin 2i$, and therefore $-k'$; and for $i > \varpi \sin 2r > \sin 2i$ or $+k'$.

44. Or again, *without assuming the value of $k_{\prime}$*, we may proceed thus : from the equation of *vis viva*,

$$(k^2 - k'^2)\frac{\sin r \cos i}{\sin i \cos r} = k_{\prime}^2;$$

from the law of equivalence,

$$(k+k')^2\frac{\cos^2 i}{\cos^2 r} = k_{\prime}^2.$$

Equating these, we have

$$(k^2 - k'^2) \sin r \cos^2 r \cos i = (k+k')^2 \sin i \cos^2 i \cos r$$
$$(k^2 - k'^2) \sin 2r = (k+k')^2 \sin 2i$$
$$k^2(\sin 2r - \sin 2i) = 2kk' \sin 2i + k'^2(\sin 2i + \sin 2r);$$

and observing that

$$2kk' \sin 2i = kk'\big( (\sin 2i - \sin 2r) + (\sin 2i + \sin 2r) \big)$$
$$(k+k')k(\sin 2r - \sin 2i) = (k+k')k'(\sin 2\,i + \sin 2r)$$
$$k(\sin 2r - \sin 2i) = k'(\sin 2r + \sin 2i)$$
$$k + k' = 2 \sin 2r, \text{ and thence } k_{\prime} = 2 \sin 2r\,\frac{\cos i}{\cos r};$$

or as before, dividing by $k$,

$$k' = \frac{\sin 2r - \sin 2i}{\sin 2r + \sin 2i}, \quad k_{\prime} = \frac{4 \sin r \cos i}{\sin 2r + \sin 2i}.$$

45. Now it is to be observed that these formulas differ from

those of Fresnel, as before given, in that the numerator of $k'$ is here *negative* for $i < \varpi$ and *positive* for $i > \varpi$.

This difference is traceable to the process by which the deduction of Fresnel's original form was effected; it being made by means of two equations (see Airy's Tract, § 129), one of which is the same as the equation of *vis viva*, viz.

$$\sin r \cos i (k^2 - k'^2) = \sin i \cos r k_i{}^2 ;$$

the other is

$$\cos i (k - k') = \cos r k_i,$$

which involves a different assumption as to the principle of equivalent vibrations, as before observed (30).

46. It will be desirable to follow the deduction of the formulas on this supposition.

For vibrations *perpendicular* to the plane of incidence, from the *vis viva*,

$$(h^2 - h'^2)\frac{\sin r \cos i}{\sin i \cos r} = h_i{}^2.$$

From *this* law of equivalence (30),

$$(h - h')^2 = h_i{}^2.$$

Hence

$$(h^2 - h'^2) \sin r \cos i = (h - h')^2 \sin i \cos r,$$

or

$$(h + h') \sin r \cos i = (h - h') \sin i \cos r$$

$$h(\sin i \cos r - \sin r \cos i) = h'(\sin r \cos i + \sin i \cos r),$$

whence

$$h' = \frac{\sin (i - r)}{\sin (i + r)}, \quad h_i = \frac{2 \sin r \cos i}{\sin (i + r)}.$$

These differ from Fresnel's *original* formulas in the *sign* of $h'$.

47. For vibrations *parallel* to the plane of incidence, on the same supposition (30) we have

$$k - k' = k_i \frac{\cos r}{\cos i};$$

and thence, as in (44),

$$(k^2 - k'^2) \sin 2r = (k - k')^2 \sin 2i ;$$

whence, in like manner,

$$(k - k')k(\sin 2i - \sin 2r) = (k - k')k'(\sin 2i + \sin 2r),$$

and thence

$$k' = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r}, \quad \text{and } k_i = \frac{4 \sin r \cos i}{\sin 2i + \sin 2r},$$

which are the formulas of Fresnel *as originally deduced*, and in which (as before) we have $+k'$ for $i < \varpi$, and $-k'$ for $i > \varpi$.

48. Here, again, we might otherwise deduce the same formulas; as before (43) assuming $k_i = 2h_p$, we should have

$$k - k' = k\frac{\cos r}{\cos i} = 2\sin 2r,$$

and $(k^2 - k'^2) = 4\sin 2r \sin 2i$ gives $k + k' = 2\sin 2i$, whence

$$k' = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r}, \text{ and } k_i = \frac{4\sin r \cos i}{\sin 2i + \sin 2r}.$$

49. In another point of view we may observe the relation subsisting between all the above formulas (generalizing a remark in Mr. Power's paper), as follows:—

Trigonometrically, we have evidently,

$$\sin^2(i+r) - \sin^2(i-r) = \sin 2i \sin 2r, \quad . \quad . \quad . \quad . \quad . \quad \text{(P)}$$

$$[\sin 2i + \sin 2r]^2 - [\sin 2i - \sin 2r]^2 = 4\sin 2i \sin 2r. \quad \text{(Q)}$$

50. Also from $\mu \sin r = \sin i$, we have the identical equation

$$4\mu \sin r \sin i \cos^2 i \cos r = 4\sin^2 i \cos^2 i \cos r,$$

or

$$\mu \cos i(\sin 2r \sin 2i) = \cos r(\sin^2 2i);$$

whence, substituting in this equation the value from (P), it becomes

$$\mu \cos i[\sin^2 (i+r) - \sin^2 (i-r)] = \cos r(\sin^2 2i);$$

or from (Q) we have

$$\mu \cos i[(\sin 2i + \sin 2r)^2 - (\sin 2i - \sin 2r)^2] = \cos r \, 4\sin^2 2i;$$

which agree with the formulas of Maccullagh, and the equation of *vis viva* on the hypothesis of equal densities.

51. Again, we have the identical equation

$$4\cos^2 i \sin i \sin r \cos r = 4\mu \sin r \cos^2 i \sin r \cos r,$$

or

$$\cos i(\sin 2i \sin 2r) = \mu \cos r(4\cos^2 i \sin^2 r);$$

whence from (P) we have

$$\cos i[\sin^2(i+r) - \sin^2(i-r)] = \mu \cos r(4\cos^2 i \sin^2 r);$$

or from (Q) we have

$$\cos i[(\sin 2i + \sin 2r)^2 - (\sin 2i - \sin 2r)^2] = \mu \cos r(16\cos^2 i \sin^2 r);$$

which agree with Fresnel's formulas, and the equation of *vis viva* on the hypothesis of increased density.

52. But thus far nothing determines *the plane of vibration*. For this purpose we must apply the law of equivalent vibrations; and in either case, since it is the *squares* of the amplitudes which enter the formulas, we may take $\pm h'$, $\pm h_p$, $\pm k'$, $\pm k_p$, according

as they fulfil the conditions with the upper or lower sign: in this way we reproduce the foregoing results, which it is needless here to repeat.

### *Experimental Evidence.*

53. That the whole series of experimental results relative to polarization are accurately represented by the foregoing formulas, has been fully substantiated by the researches of Fresnel, Arago, Brewster, and others; but these changes afford no distinctive test between the several theories here adverted to.

54. It may be desirable more particularly here to notice the interpretation of *the change of sign* in these formulas.

In general it is obvious, that in the elementary formula

$$u = h \sin \frac{2\pi}{\lambda} (vt - x);$$

if we suppose $x$ increased to $x + \frac{\lambda}{2}$, we have

$$u' = h \sin \frac{2\pi}{\lambda} \left( vt - x - \frac{\lambda}{2} \right) = h \sin \left[ \frac{2\pi}{\lambda} (vt - x) - \pi \right] = -h \sin \frac{2\pi}{\lambda} (vt - x),$$

or that a *difference of sign* indicates a *change of* 180° *in phase*, and that this is equivalent to *a change of* $\frac{\lambda}{2}$ *in route.*

55. This conclusion includes the solution of the difficulty as to the loss or gain of half a wave-length between reflexion at the first surface of a dense medium represented by $\sin (i - r)$, where we have $i > r$; and at the second, where we have $i < r$, which gives $-\sin (i - r)$,—a difficulty which so long embarrassed the early history of the theory, and which it is perhaps desirable to state thus clearly, though of so elementary a character, as it would sometimes appear to be still felt.

There are, however, other cases when this change of sign is material.

### *Intensities of Light.*

56. The intensities of the reflected and refracted rays at different incidences, as compared with that of the incident light, are measured by the *vires vivæ*, that is, by the vibrating masses multiplied by the squares of the amplitudes or velocities.

Thus, considering the intensity of the incident ray as unity, for (H) let

$$I = m h'^2, \quad I_{\prime} = m_{\prime} h_{\prime}^2,$$

and for (K) let

$$J = m k'^2, \quad J_{\prime} = m_{\prime} k_{\prime}^2;$$

or dividing,

$$I = h'^2, \quad I_{_{\prime}} = \frac{m_{_{\prime}}}{m} h_{_{\prime}}^2,$$

$$J = k'^2, \quad J_{_{\prime}} = \frac{m_{_{\prime}}}{m} k_{_{\prime}}^2;$$

whence, calculating the values of $h'$, $h_{_{\prime}}$ and $k'$, $k_{_{\prime}}$ for the several incidences, we have the intensities directly.

But from the equation of *vis viva* we have

$$(1 - h'^2) = \frac{m_{_{\prime}}}{m} h_{_{\prime}}^2,$$

$$(1 - k'^2) = \frac{m_{_{\prime}}}{m} k_{_{\prime}}^2.$$

Thus, knowing only the values $h'$ and $k'$, we obtain at once those of the refracted intensities, instead of having to calculate $h_{_{\prime}}$ and $k_{_{\prime}}$, which is a troublesome process; thus we take simply

$$I_{_{\prime}} = 1 - h'^2, \quad J_{_{\prime}} = 1 - k'^2.$$

And this, it will be observed, is independent of any hypothesis as to *densities*.

57. If we calculate $I_{_{\prime}}$ and $J_{_{\prime}}$ *directly* as above, the values of the amplitudes will differ according to the hypotheses of the density: but then we have to observe, as before, that (denoting Fresnel's values by roman letters) we have (14) and (19)

$$h_{_{\prime}} = \mathrm{h}_{_{\prime}} \mu, \quad k_{_{\prime}} = \mathrm{k}_{_{\prime}} \mu, \quad \frac{m_{_{\prime}}}{m} = \frac{\mathrm{m}_{_{\prime}}}{\mathrm{m}} \frac{1}{\mu^2},$$

and thus

$$\frac{m_{_{\prime}}}{m} h_{_{\prime}}^2 = \frac{\mathrm{m}_{_{\prime}}}{\mathrm{m}} \mathrm{h}_{_{\prime}}^2, \quad \frac{m_{_{\prime}}}{m} k_{_{\prime}}^2 = \frac{\mathrm{m}_{_{\prime}}}{\mathrm{m}} \mathrm{k}_{_{\prime}}^2.$$

Thus, either way, the *calculated intensities will be the same on either hypothesis*.

58. To trace the intensities at different incidences :—

(1.) At the perpendicular incidence $i = 0$ and $r = 0$. Thus the formulas become $h' = \dfrac{0}{0}$, $k' = \dfrac{0}{0}$. But the actual values may be found, since at perpendicular incidences we have (16),

For equal densities, $\dfrac{m_{_{\prime}}}{m} = \dfrac{1}{\mu}$ ;

For increased density, $\dfrac{m_{_{\prime}}}{m} = \mu.$

In either case the formula $h' = \dfrac{m - m_{_{\prime}}}{m + m_{_{\prime}}}$ will give

$$h'^2 = \frac{(\mu - 1)^2}{(\mu + 1)^2}.$$

This was Dr. Young's formula, deduced in an early stage of these inquiries.

If we suppose $\mu = 1.5$, this will give

$$I = h'^2 = \left(\frac{.5}{2.5}\right)^2 = .04,$$

$$I_{,} = 1 - h'^2 = .96.$$

Also
$$h' = h'\frac{\cos(i-r)}{\cos(i+r)} = h',$$

whence
$$J = k'^2 = h'^2 = .04, \quad J_{,} = .96.$$

(2.) For the angle of complete polarization $i + r = 90°$,
$\sin(i-r) = \cos 2r, \quad \sin(i+r) = 1, \quad \tan i = \mu = 1.5 = \tan 56° \; 19',$
$$I = h'^2 = \cos^2 2r, \quad J = 1 - h'^2 = \sin^2 2r,$$

$$J = k'^2 = \frac{\tan^2(i-r)}{\infty} = 0, \quad J_{,} = 1 - k'^2 = 1.$$

(3.) In the limit of oblique incidence,
$$i = 90°, \quad \sin(i-r) = \cos r, \quad \sin(i+r) = \cos r,$$
$$I = h'^2 = 1, \quad J = 1 - h'^2 = 0,$$
$$\tan(90-r) = \tan(90+r),$$
$$I_{,} = k'^2 = 1, \quad J_{,} = 1 - k'^2 = 0.$$

(4.) For these and other incidences generally, the following table will show the *approximate* intensities.

*Intensities at different Incidences.*

| $\mu = 1.5$. | | Formulas H. | | Formulas K. | |
|---|---|---|---|---|---|
| *i.* | *r.* | $I = h'^2$. | $I_{,} = (1-h'^2)$. | $J = k'^2$. | $J_{,} = (1-k'^2)$. |
| 0 ° ' | 0 ° ' | ·040 | ·960 | ·040 | ·960 |
| 20 | 13 10 | ·047 | ·953 | ·034 | ·966 |
| 40 | 25 22 | ·077 | ·923 | ·014 | ·986 |
| 56 19 | 33 41 | ·152 | ·848 | ·000 | 1·000 |
| 60 | 35 16 | ·176 | ·824 | ·002 | ·998 |
| 80 | 41 2 | ·515 | ·485 | ·197 | ·803 |
| 90 | 41 49 | 1·000 | ·000 | 1·000 | ·000 |

59. The necessary imperfections of photometry preclude any accurate verification of these numerical results; but by throwing the two images of a round hole in an opake plate covering one end of a rhomboid of iceland spar on a surface of glass, at different incidences, the eye can readily compare the intensities in some of the more marked cases, both of the reflected and the transmitted rays, of the two beams polarized in planes at right angles to each other; which show a general agreement with theory.

60. The question whether the plane of vibration is parallel or perpendicular to the plane of polarization, has been lately discussed at large by M. Haidinger, who, after an extended comparative view of the experimental consequences involved in connexion with various optical phænomena *on either supposition,* decides in favour of the superior simplicity and consistency of *Fresnel's* view, that the plane of vibration is *perpendicular* to the plane of polarization. (See Phil. Mag. March 1856, vol. xi. p. 242, No. 71. The original is in Poggendorff's *Annalen,* Oct. 1855. See also Silliman's Journal, Jan. 1856.)

61. A very refined and ingenious suggestion for a direct " *experimentum crucis* " was made by Mr. J. A. Dale to the British Association in 1846, but practical difficulties appear to stand in the way of its application. (See ' Report,' 1846, Section A.)

62. But a more recent result of Prof. Stokes is perfectly conclusive on the question of the direction of the plane of vibration. In his paper " On the Dynamical Theory of Diffraction " (Cambridge Transactions, vol. ix. part 1, 1849), he has established theoretically the conclusion, that the *plane of vibration of a diffracted ray previously polarized, is in general different from that of the incident, except when perpendicular, and when parallel, to the plane of diffraction.* For intermediate positions their directions are connected by a simple law, which may be expressed by saying, that if the arcs of inclination of the vibrations to the perpendicular to the plane of diffraction be measured on two semicircles intersecting in a common diameter coinciding with that perpendicular, and inclined to each other at the angle of diffraction, then the arc of inclination of the *incident* vibration, measured from the point of intersection, being taken as *longitude,* that of the *diffracted* vibration will be the corresponding *right ascension.*

If the two semicircles, whose planes are respectively perpendicular to the directions of the incident and the diffracted rays, be inclined at an angle $\theta$, equal to the angle of diffractive deviation, and if $\alpha_d$ and $\alpha_i$ be the inclinations of the diffracted and incident vibrations measured from the intersection $0°$, by Mr. Stokes's theory they are connected by the equation

$$\tan \alpha_d = \cos \theta \tan \alpha_i,$$

which leads to the construction here represented by the arc of a great circle passing through the extremities of $\alpha_d$ and $\alpha_i$ perpendicular to the plane of $\alpha_d$. Whence it follows, that *near the intersection* $(0°)$,
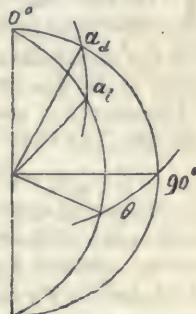
that is the *perpendicular* position, the changes in the inclination of the diffracted ray will be much less rapid, or the indications more *crowded* together, than near the *parallel* position (90°), or the plane of diffraction.

But we can *observe* the changes only in the plane of *polarization*. If, then, these changes are more slow near the *perpendicular* (0°), the planes of *polarization* are *parallel* to those of *vibration*; if near the *parallel* (90°), then they are *perpendicular* to those of vibration.

A series of experiments of the most elaborate and accurate kind unequivocally show *the latter to be the fact.*

63. *Thus experiment obliges us to adopt Fresnel's hypothesis of vibrations perpendicular to the plane of polarization,* and by consequence (as here shown) *either increased density or some other property expressed by the same law.*

In thus being compelled to relinquish the hypothesis of equal density, or at least the formulas expressing it, we do not, in fact, sacrifice anything in point of simplicity, the same amount of analysis being requisite to deduce the expressions on either supposition; and in giving up the beautiful geometry of Maccullagh, we do ample justice to his more substantial discoveries,—the general laws of equivalence of vibrations (whatever difference may arise on a subordinate point), and their connexion with the principle of conservation of *vis viva*, on which the whole theory reposes. There are, however, some other points hinted at in what precedes, which may demand further inquiry at a future opportunity.

---

II. *On the Cones which pass through a given Curve of the Third Order in Space. By* A. CAYLEY, *Esq.*[*]

THE following investigation is connected with the theory of the cubic $(a, b, c, d \Upsilon x, y)^3$, and in particular with a theorem that the determinant formed with the second differential coefficients of the discriminant gives the square of the discriminant.

Consider the coefficients $a, b, c, d$ as linear functions of coordinates, the equations

$$ac - b^2 = 0, \quad bc - ad = 0, \quad bd - c^2 = 0$$

(equivalent, of course, to two equations) belong to a curve of the third order in space, the edge of regression of the developable surface obtained by putting the discriminant equal to zero, or which has for its equation

$$-a^2d^2 + 6abcd - 4ac^3 - 4b^3d + 3b^2c^2 = 0.$$

And, moreover, the above forms are the general representations

* Communicated by the Author.

of any curve of the third order, and developable surface of the fourth order. The question arises, to find the equation of the cone of the third order having an arbitrary point for its vertex, and passing through the curve of the third order. This may be done by Joachimsthal's method : let $\alpha$, $\beta$, $\gamma$, $\delta$ be the values of $a$, $b$, $c$, $d$ at the vertex of the cone, and in the equations of the curve, say in

$$ac - b^2 = 0, \quad bd - c^2 = 0,$$

for $a$, $b$, $c$, $d$ write $ua + v\alpha$, $ub + v\beta$, $uc + v\gamma$, $ud + v\delta$; if from the equations so obtained $u$, $v$ are eliminated, the resulting equation will be that of the cone of the third order.

The substitutions in question give

$$L u^2 + 2M uv + N v^2 = 0$$
$$L'u^2 + 2M'uv + N'v^2 = 0,$$

where

$$L = 2(ac - b^2), \quad M = a\gamma + c\alpha - 2b\beta, \quad N = 2(\alpha\gamma - \beta^2),$$
$$L' = 2(bd - c^2), \quad M' = b\delta + d\beta - 2c\gamma, \quad N' = 2(\beta\delta - \gamma^2).$$

The result of the elimination is

$$4(LN - M^2)(L'N' - M'^2) - (LN' + L'N - 2MM')^2 = 0,$$

and we have

$$LN - M^2 = 4(b\gamma - c\beta)(a\beta - b\alpha) - (c\alpha - a\gamma)^2,$$
$$L'N' - M'^2 = 4(c\delta - d\gamma)(b\gamma - c\beta) - (b\delta - d\gamma)^2,$$
$$LN' + L'N - 2MM' = 4(b\gamma - c\beta)^2 + 4(a\beta - b\alpha)(c\delta - d\gamma)$$
$$+ 2(c\alpha - a\gamma)(b\delta - d\beta).$$

Write for shortness,

$$b\gamma - c\beta = l, \quad c\alpha - a\gamma = m, \quad a\beta - b\alpha = n,$$
$$a\delta - d\alpha = f, \quad b\delta - d\beta = g, \quad c\delta - d\gamma = h,$$

values which give $lf + mg + nh = 0$. Then forming the expression

$$4(4ln - m^2)(4hl - g^2) - (4l^2 + 4nh + 2mg)^2,$$

this is equal to*

$$-16l(l^3 - nfh + lmg - 2lnh + ng^2 + m^2h).$$

---

\* With respect to the occurrence of the factor $l (= b\gamma - c\beta)$, it is worth noticing, that, putting $b = k\beta$, $c = k\gamma$, we have identically

$$L u^2 + 2M uv + Nv^2 = 2[(a\gamma - b\beta)u + (\alpha\gamma - \beta^2)v](ku + v),$$
$$L'u^2 + 2M'uv + N'v^2 = 2[(\beta\delta - c\gamma)u + (\beta\delta - \gamma^2)v](ku + v),$$

*i. e.* the two functions will contain a common factor if $b = k\beta$, $c = k\gamma$, or what is the same thing, if $b\gamma - c\beta = 0$. But if the functions contain a common factor, their resultant vanishes, *i. e.* the resultant will vanish in virtue of the relation $b\gamma - c\beta = 0$, or what is the same thing, $b\gamma - c\beta$ is a factor of the resultant.

And the equation of the cone is

$$l^3 - nfh + lmg - 2lnh + ng^2 + m^2h = 0.$$

Or substituting and expanding,

$$(\alpha\delta^2 - \gamma^3)b^3 - (\alpha^2\delta - \beta^3)c^3 - \delta(\beta\delta - \gamma^2)ab^2 + \alpha(\alpha\gamma - \beta^2)dc^2$$
$$+ \delta(\beta\delta - \gamma^2)a^2c - \alpha(\alpha\gamma - \beta^2)d^2b + 2\delta(\alpha\gamma - \beta^2)ac^2 - 2\alpha(\beta\delta - \gamma^2)db^2$$
$$- \gamma(\beta\delta - \gamma^2)a^2d + \beta(\alpha\gamma - \beta^2)ad^2 + 3\gamma(\beta\gamma - \alpha\delta)b^2c - 3\beta(\beta\gamma - \alpha\delta)bc^2$$
$$+ \delta(\beta\gamma - \alpha\delta)abc - \alpha(\beta\gamma - \alpha\delta)dbc + (\alpha\gamma\delta - 3\beta\gamma^2 + 2\beta^2\delta)abd$$
$$- (\alpha\beta\delta - 3\beta^2\gamma + 2\alpha\gamma^2)acd = 0.$$

Now putting

$$bd - c^2 = p, \quad bc - ad = q, \quad ac - b^2 = r,$$

and in like manner

$$\beta\delta - \gamma^2 = P, \ \beta\gamma - \alpha\delta = Q, \ \alpha\gamma - \beta^2 = R,$$

and reducing, the final result may be expressed in the form

$$P\{ \quad - \quad 2\alpha bp + (\gamma a - 2\beta b)q + (\delta a - \gamma c)r\}$$
$$+ Q\{ \quad - \quad \alpha cp + (\gamma b - \beta c)q \quad + \quad \delta br\}$$
$$+ R\{ -(\alpha d - \beta c)p - (\beta d - 2\gamma c)q \quad + \quad 2\delta cr\}$$
$$= 0,$$

where $a$, $b$, $c$, $d$ are current co-ordinates, and $p$, $q$, $r$ are quadratic functions of $a$, $b$, $c$, $d$. The equation is (as it should be) satisfied by the equations ($p = 0$, $q = 0$, $r = 0$) of the given curve; it is also satisfied *per se* when $P = 0$, $Q = 0$, $R = 0$, *i. e.* when the vertex is a point on the curve; this indicates a change in form of the equation, and in fact the cone is in this case of the second order only. Suppose that the co-ordinates of the vertex are in this case given by $\dfrac{\alpha}{\beta} = \dfrac{\beta}{\gamma} = \dfrac{\gamma}{\delta} = \dfrac{1}{\sigma}$ ($\sigma$ an arbitrary quantity), it may be easily shown that the equation of the cone is

$$p + \sigma q + \sigma^2 r = 0 ;$$

or at full length,

$$(bd - c^2) + \sigma(bc - ad) + \sigma^2(ac - b^2) = 0.$$

In fact this equation is evidently that of a surface of the second order passing through the curve; and there is no difficulty in showing that it is a cone.

2 Stone Buildings,
　May 1, 1856.

III. *On a peculiar Power possessed by Porous Media (Sand and Charcoal) of removing Matter from* Solution *in Water.* By HENRY M. WITT, *F.C.S., Assistant Chemist to the Government School of Applied Science\*.*

VARIOUS methods have been employed at different times for the purification of water for the supply of towns, but none has been found so practically convenient and efficacious as simple filtration through porous media, such as sand; moreover, charcoal being known to possess a peculiar power of removing *organic* matters from solution, this substance has been suggested and occasionally employed either as a substitute for, or an auxiliary to, ordinary sand filtration.

The following experiments were undertaken with the view of ascertaining by chemical analysis the more precise nature of the effects produced upon ordinary river-water, such as that of the Thames, by its passage through filters composed of these media respectively, and of comparing their powers; but it is believed that the results obtained possess an interest extending considerably beyond the question to assist in the solution of which they were made.

Before proceeding to the construction of experimental filters, I availed myself of the kindness of Mr. James Simpson, engineer to the Chelsea Waterworks Company, to investigate the results obtained by that Company's system of filtration as carried on up to the present time at Chelsea; and I have much pleasure in embracing this opportunity of expressing my obligation to this gentleman, and specially also to his son, Mr. James Simpson, jun., to whom I am greatly indebted for his very able cooperation throughout this inquiry.

The system of purification adopted by the Chelsea Waterworks Company at their works at Chelsea, consisted hitherto (for the supply has by this time commenced from Kingston) in pumping the water up out of the river into subsiding reservoirs, where it remained for six hours; it was then allowed to run on to the filter beds. These are large beds of sand and gravel, each exposing a filtering surface of about 270 square feet, and the water passes through them at the rate of about $6\frac{1}{4}$ gallons per square foot of filtering surface per hour, making a total quantity of 1687·5 gallons per hour through each filter.

The filters are composed of the following strata in a descending order:—

---

\* Communicated by the Author.

|  |  |  |  |  |  |  | ft. | in. |
|---|---|---|---|---|---|---|---|---|
| No. 1. | Fine sand | . | . | . | . | . | 2 | 6 |
| 2. | Coarser sand | . | . | . | . | . | 1 | 0 |
| 3. | Shells. | . | . | . | . | . | 0 | 6 |
| 4. | Fine gravel | . | . | . | . | . | 0 | 3 |
| 5. | Coarse gravel | . | . | . | . | . | 3 | 3 |

These several layers of filtering materials are not placed perfectly flat, but are disposed in waves, as seen in the sectional drawing; and below the convex curve of each undulation is placed a porous earthenware pipe, which conducts the filtered water into the mains for distribution.

*Chelsea Waterworks, Thames Bank.   Transverse Section of Filter.
Downward Filtration.*
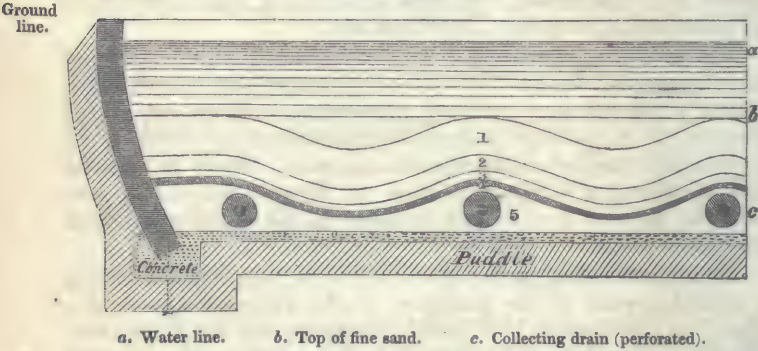


a. Water line.     b. Top of fine sand.     c. Collecting drain (perforated).
*Note.*—Depth of water over the sand =4 ft. 6 in.

The upper layer of sand is renewed about every six months, but the body of the filter has been in use for about twenty years.

Samples of water were taken and submitted to examination—

1st. From the reservoir into which the water was at the time being pumped from the middle of the river.

2nd. From the cistern after subsidence and filtration.

Experiments of this kind were made on three different occasions, viz. on the 12th of September and the 29th of December, 1855; and also on the 10th of May 1856; and the results are embodied in Tables I., II. and III., each containing four columns,—No. 1 showing the quantities of the several substances originally present, represented in grains in the imperial gallon (70,000 grains) of water; No. 2, the amounts present after filtration; No. 3, the actual quantities separated in grains in the gallon of water; and No. 4, the per-centage ratio which the amounts separated bear to the quantities originally present.

*Results of Filtration through Sand at the Chelsea Waterworks.*

TABLE I.—September 12, 1855.

|  | 1.<br>Originally present. | 2.<br>Present after filtration. | 3.<br>Amount separated. | 4.<br>Per-centage ratio of sepa-rated matter. |
|---|---|---|---|---|
| Total solid residue (including suspended matter) ......... } | 65·527 | 41·290 | 24·237 | 36·98 |
| Organic matter ................ | 1·240 | 0·674 | 0·566 | 45·63 |
| Total mineral matter ......... | 64·287 | 40·616 | 23·671 | 36·82 |
| Suspended matter ............. | 16·112 | ............... | nearly | all. |
| Total soluble salts ............ | 48·175 | 40·616 | 7·559 | 15·69 |
| Chlorine ....................... | 12·240 | 11·140 | 1·100 | .... |
| Chloride of sodium............ | 20·170 | 18·357 | 1·820 | 9·23 |

TABLE II.—December 29, 1855.

|  | 1.<br>Originally present. | 2.<br>After filtration. | 3.<br>Amount separated. | 4.<br>Per-centage ratio of sepa-rated matter. |
|---|---|---|---|---|
| Total solid residue (including suspended matter) ......... } | 31·467 | 24·153 | 7·314 | 23·24 |
| Organic matter ................ | 2·375 | 1·889 | 0·486 | 20·46 |
| Total mineral matter......... | 29·092 | 22·264 | 6·828 | 23·47 |
| Suspended matter ............. | 7·035 | 1·403 | 5·632 | 80·05 |
| Total dissolved salts ......... | 22·057 | 20·861 | 1·196 | 5·42 |
| Lime .......................... | 8·675 | 8·402 | 0·273 | 3·14 |
| Chlorine ....................... | 1·667 | 1·546 | 0·121 | 7·25 |
| Chloride of sodium ......... | 2·747 | 2·547 | 0·200 | 7·28 |
| Sulphuric acid ............... | 2·962 | 2·372 | 0·590 | 19·92 |

TABLE III.—May 10, 1856.

|  | 1.<br>Originally present. | 2.<br>After filtration. | 3.<br>Amount separated. | 4.<br>Per-centage ratio of sepa-rated matter. |
|---|---|---|---|---|
| Total solid residue (including suspended matter) ......... } | 55·60 | 22·85 | 32·75 | 58·90 |
| Organic matter ............... | 4·05 | 1·349 | 2·70 | 66·66 |
| Total mineral matter ......... | 51·55 | 21·501 | 30·049 | 58·29 |
| Suspended matter ............. | 28·93 | 2·285 | 26·645 | 92·10 |
| Total dissolved salts ......... | 22·62 | 19·216 | 3·404 | 15·04 |
| Lime .......................... | 8·719 | 8·426 | 0·293 | 3·36 |

By these analytical results (if entitled to confidence, and I may state that all due care and attention was given to ensure their accuracy) it is shown that this process of mere sand filtration is one of more importance, and of a more peculiar and interesting character, than has generally been supposed.

It has been asserted as a principle that sand filtration *can*

only remove bodies mechanically suspended in water, but I am not aware that this statement has been established by experiment; in fact, I am not acquainted with any published analytical examination of the effects of sand filtration.

These experiments supply the deficiency, and show moreover, that these porous media are not only capable of removing suspended matter (80 to 92 per cent.), but even of separating a *certain appreciable quantity of the* SALTS *from* SOLUTION *in water* ! viz. from 5 to 15 per cent. of the amount originally present, 9 to 19 per cent. of the common salt, 3 per cent. of the lime, and 5 of the sulphuric acid.

It is curious also that the proportion of matter removed in this way depends to a certain extent upon the degree of impurity of the water; the greater the quantity of matter originally present in the water, the larger the per-centage ratio of the salts removed, *e. g.* :—

|  | In Sept. | In Dec. | In May. |
|---|---|---|---|
| Total impurity of water . . . | 65·527 | 31·467 | 55·90 |
| Per-centage of salts removed . | 15·69 | 5·42 | 15·04 |

This point will, however, become more apparent by comparing these experiments with those made at Kingston, where the water is much purer, to which I shall have occasion hereafter more particularly to refer.

This fact, of the power possessed by sand and other porous media, which rests for demonstration not only upon the three preceding experiments, but also upon others to be presently described, is one of great importance, not only in a hygienic and œconomic point of view as relating to the great question of water supply, but also in its bearings upon agriculture and geology.

It is possible that soils may remove matters from solution in water, not only by decompositions between the contained salts and the aluminous silicates of the soil (as demonstrated by Mr. Way), but also in virtue of this peculiar action.

Again, water containing considerable quantities of saline matter in solution may, by merely percolating through *great masses* of porous strata during *long periods,* be gradually deprived of its salts to such an extent as probably to render even sea-water fresh.

This may in fact be one of the causes contributing towards the production of freshwater springs which ebb and flow with the tide in the vicinity of the sea : for instance, Darwin, in the ' Voyage of the Beagle' (vol. iii. p. 545), mentions that on Keeling Island, one of the coral reefs near the coast of Sumatra, there are freshwater wells which ebb and flow with the tide. Mr. Darwin, however, suggests another explanation of this

phænomenon; he says that " at first sight it would appear not
a little remarkable that the fresh water should regularly ebb
and flow with the tide. We must believe that the compressed
sand or porous coral rock acts like a sponge, and that the rain-
water which falls on the ground being specifically lighter than
salt, floats on its surface, and is subject to the same movements.
There can be no actual attraction between salt and fresh water,
and the spongy texture must tend to prevent all admixture
from slight disturbances. On the other hand, where the foun-
dation consists only of loose fragments, upon a well being dug
salt or brackish water enters, of which fact we saw an instance
on this small island."

Now I would not presume to deny the accuracy of this ex-
planation, still it is possible that the phænomenon may be a
mixed one, and that the peculiar power which I have found to
be possessed by porous media may contribute towards the re-
sult by abstracting a portion (at least) of the salts from the sea-
water during its passage through the porous rock.

Moreover, there are certain difficulties in Mr. Darwin's ex-
planation. If the sand did not possess the power of removing
salts from solution, to a certain extent at all events, the rain-
water inside would tend, more or less rapidly, to become salt by
the commixture with it of the sea-water from without, which
would take place through the porous diaphragm so much the
more readily on account of their difference of density, in virtue
of the " osmotic force " so ably investigated by Professor Graham.
Indeed, the last fact mentioned by Mr. Darwin, viz. that when
the wall consists of loose fragments the wells are salt, tends to
show that it is only the very finely divided sand, the porous me-
dium offering a considerable extent of surface, which possesses
the power of removing the salts, for if the non-commixture of
the salt and fresh water arose only from their difference of
density, the coarseness of the diaphragm would not materially
affect it.

As to the *cause* of this peculiar action, the only suggestion
which I may venture to offer is, that it possibly arises from the
weak chemical affinity in virtue of which salts are held in solu-
tion in water, being overcome by a physical force, the attraction
of cohesion, or more properly adhesion, exerted by the greatly
extended surface of the porous body; this is not a solitary in-
stance of such weak chemical forces as solution being overcome
by a physical attraction. Gmelin* instances several other ex-
amples of the power of the physical force of *" adhesion "* to over-
come weak chemical attractions, *e. g.*: " When vinegar is filtered
through quartz sand, the first portion of liquid that runs through

* Handbook of Chemistry, Cavendish Society's Translation, vol. i. p. 114.

is robbed of almost all its acid, and the vinegar does not pass through unchanged until the sand has become well charged with acid. Potato brandy diluted with water, and filtered through quartz sand, yields at first pure water, then a mixture of water and alcohol deprived of its fusel oil, and lastly, the original mixture unaltered : wood shavings also deprive vinegar of nearly all its acid ; charcoal acts still more powerfully\*."

Now it is possible that these being organic compounds they may be oxidized, even into carbonic acid and water, under these circumstances ; but whatever difference of opinion may arise respecting the *cause*, this cannot affect the simple experimental fact.

It will naturally be asked, if sand possess this power, Are not the salts removed from the water accumulated in the sand, and would not this throw light upon the mode of formation of sand-stone conglomerate rocks ? To enable me to reply to this question, I have in my possession samples of sand, before use, and also after employment in the middle of the filter at Chelsea for no less than twenty years, and their analyses will form the subject of a future communication ; pressure of other duties having hitherto prevented me from the completion of this important point in the inquiry.

But, as before mentioned, the preceding are not the only experiments which I have made on this subject : the following were performed at Kingston, where the water is far purer, more particularly for the purpose of ascertaining the effects of charcoal and comparing its powers with those of sand.

At the time these experiments were in progress, the Company's filter-beds at Kingston, now just completed, were unfinished, and consequently, in order to ascertain the effects both of sand and charcoal on the comparatively pure water of Kingston, it became necessary to construct smaller experimental filters.

The earlier experiments were upon the effects of charcoal, and several preliminary trials were made for the purpose of ascertaining the form of filter and rate of filtration which would nearest approximate to the operation as carried on on the large scale.

In the first of these experiments, to which attention may be drawn, the water was pumped from the river into a cistern of slate, where a deposition of the greater portion of the suspended matter took place ; it was then conducted into the upper part of the right-hand compartment of a nearly cubical wooden cistern, this right-hand compartment being separated from the one on the left by a porous diaphragm, the walls of which were of wire-gauze, and the interior filled with fragments of oak charcoal, about the size of a hazel nut to a walnut : this charcoal dia-

\* Wagenmann, Poggendorff's *Annalen*, xxiv. 600.

phragm exposed a filtering surface of about 4·38 square feet; the filtration was lateral and descending, the water entering at the upper part of the right-hand compartment, and passing out at the lower extremity of the left-hand division; the rate of filtration being half a gallon per minute.

A sample of the water was analysed before passing into the filter, and after coming from it, after the continuance of the operation for a certain number of hours. The results were as follows:—

TABLE IV.

| | Original water used. | After 67 hours' action. | | | After 91 hours' action. | | |
|---|---|---|---|---|---|---|---|
| | | Comparison. | Amount separated. | Per-centage ratio of quantity separated. | Comparison. | Amount separated. | Per-centage ratio of quantity separated. |
| Total residue ...... | 23·949 | 23·668 | 0·281 | 1·17 | 22·076 | 1·873 | 7·82 |
| Mineral salts ...... | 22·519 | ...... | ...... | ...... | 21·618 | 0·901 | 4·00 |
| Organic matter ... | 1·430 | 0·475 | 0·955 | 66·78 | 0·458 | 0·972 | 68·00 |
| Chlorine ............ | 1·054 | ...... | ...... | ...... | 0·9731 | ·081 | 7·685 |
| Chloride of sodium. | 1·737 | ...... | ...... | ...... | 1·603 | 0·134 | 7·714 |

| | | After 115 hours' action. | | | After 139 hours' action. | | |
|---|---|---|---|---|---|---|---|
| Total residue ...... | 23·949 | 23·012 | 0·937 | 3·91 | 23·411 | 0·538 | 2·242 |
| Mineral salts ...... | 22·519 | ...... | ...... | ...... | 22·44 | 0·079 | 3·66 |
| Organic matter ... | 1·430 | 0·620 | 0·810 | 56·67 | 0·971 | 0·459 | 32·10 |
| Chlorine ............ | 1·054 | 0·885 | 0·169 | 16·03 | 0·973 | 0·081 | 9·73 |
| Chloride of sodium. | 1·737 | 1·457 | 0·280 | 16·10 | 1·603 | 0·134 | 9·8 |

This experiment exhibits more strikingly perhaps than any of the others, the very curious circumstance that in the employment of charcoal the amount of impurity separated by it goes on increasing to a certain point, and then diminishes; to which, however, I must again more particularly allude after describing the experiment comparing sand and charcoal simultaneously; it demonstrates also very satisfactorily the power possessed by charcoal, like sand, of removing not only suspended matters, but also soluble salts, *e. g.* chloride of sodium: but to this point I shall again refer.

Although very satisfactory as showing the power of charcoal under the circumstances of the experiment, yet it being probable that by employing the charcoal in lumps, as was done, it had not a fair chance of exhibiting its peculiar properties, inasmuch as only very imperfect contact was effected between the impurities contained in the water and the particles of charcoal, it was resolved to try an experiment with charcoal in powder, or nearly so, in granules of the size of a pea. Moreover, even then we should have succeeded only in proving the *absolute* effect of

charcoal, for we had learnt that the results thus obtained did not admit of comparison with those of filtration through sand at Chelsea, on account of the great difference in the original composition of the water; therefore, in order to establish at the same time the absolute effect of charcoal as a filtering medium upon the purer water at Kingston, and also to compare it with that of sand, two experiments were made simultaneously with the same water, the one of filtration through charcoal alone, and the other through sand alone, both under precisely similar circumstances. In both these experiments the filtration was not effected laterally as in the foregoing case, but *downwards* in the usual way through beds of the same size of the respective filtering media contained in wooden troughs, the water being admitted at the top and passing off from below.

The sand-filter had an area of 4 square feet, and consisted of the following materials:—

|  | ft. | in. |
|---|---|---|
| Fine sand . . . . . . . . | 1 | 9 |
| Shells . . . . . . . . . | 0 | $1\frac{1}{2}$ |
| Gravel . . . . . . . . . | 0 | $1\frac{1}{2}$ |
| Coarse gravel . . . . . . | 0 | 9 |
|  | 2 | 9 |

The rate of filtration being half a gallon per minute.

TABLE V.—Results of Sand Filtration.

| | Original water used. | After 23 hours' action. | | | After 120 hours' action. | | |
|---|---|---|---|---|---|---|---|
| | | Com-parison. | Amount sepa-rated. | Per-cent-age ratio of quantity separated. | Com-parison. | Amount sepa-rated. | Per-cent-age ratio of quantity separated. |
| Total residue ...... | 24·578 | 23·87 | 0·708 | 2·88 | 23·69 | 0·888 | 3·613 |
| Mineral salts ...... | 23·687 | 22·858 | 0·829 | 3·50 | 23·04 | 0·647 | 2·73 |
| Organic matter ... | 0·8906 | 1·012 | ...... | ...... | 0·648 | 0·2426 | |
| Suspended matter. | 3·509 | 2·663 | 0·846 | 24·109 | | | |
| Chlorine ............ | 0·862 | ...... | ...... | ...... | 0·671 | 0·191 | 22·16 |
| Chloride of sodium. | 1·420 | ...... | ...... | ...... | 1·105 | 0·315 | 22·11 |

| | Original water used. | After 240 hours' action. | | | After 376 hours' action. | | |
|---|---|---|---|---|---|---|---|
| Total residue ...... | 24·578 | 22·534 | 2·044 | 8·316 | 22·507 | 2·071 | 8·426 |
| Mineral salts ...... | 23·687 | 21·517 | 2·170 | 9·161 | 21·698 | 1·989 | 8·397 |
| Organic matter ... | 0·8906 | 0·917 | ...... | ...... | 0·809 | | |
| Suspended matter. | 3·509 | 1·88 | 1·629 | 46·423 | 1·584 | 1·925 | 54·85 |
| Chlorine ............ | 0·862 | 0·674 | 0·188 | 21·8 | | | |
| Chloride of sodium. | 1·420 | 1·110 | 0·310 | 21·8 | | | |

Apart from its special interest, as compared with the following experiment made simultaneously through charcoal, the following

points are in themselves remarkable in the results obtained by this filtration through sand :—

1st. That the filter continued increasing in efficacy even till the conclusion of the experiment, *i. e.* for 376 hours, not having lost any of its power when the experiment was terminated.

2nd. That no weighable quantity of dissolved organic matter was removed by the sand in this experiment ; but it must be remembered that the quantity originally present was but small.

3rd. Its power of removing soluble salts was considerable ; as a maximum, 21 per cent. of the common salt being separated.

TABLE VI.—Results of Charcoal Filtration.

|  | Original water used. | After 72 hours' action. | | | After 120 hours' action. | | |
|---|---|---|---|---|---|---|---|
|  |  | Comparison. | Amount separated. | Per-centage ratio of quantity separated. | Comparison. | Amount separated. | Per-centage ratio of quantity separated. |
| Total residue ...... | 24·578 | 22·13 | 2·448 | 9·906 | 21·644 | 2·934 | 11·93 |
| Mineral matter ... | 23·687 | 21·375 | 2·312 | 9·76 |  |  |  |
| Organic matter ... | 0·8906 | 0·755 | 0·1356 | 15·22 |  |  |  |
| Suspended matter. | 3·509 | ...... | ...... | ...... | 3·06 | 0·449 | 12·79 |
| Chlorine ............ | 0·862 |  |  |  |  |  |  |
| Chloride of sodium. | 1·420 |  |  |  |  |  |  |

|  | Original water used. | After 240 hours' action. | | | After 376 hours' action. | | |
|---|---|---|---|---|---|---|---|
| Total residue ...... | 24·578 | 20·821 | 3·757 | 15·28 | 21·374 | 3·204 | 13·03 |
| Mineral matter ... | 23·687 | ...... | ...... | ...... | 20·604 | 3·033 | 12·34 |
| Organic matter ... | 0·8906 | ...... | ...... | ...... | 0·770 | 0·1206 | 13·54 |
| Suspended matter. | 3·509 | 2·79 | 0·719 | 20·48 |  |  |  |
| Chlorine ............ | 0·862 |  |  |  |  |  |  |
| Chloride of sodium. | 1·420 |  |  |  |  |  |  |

On comparing this experiment with the preceding, the following points come out as showing the difference between the effects of sand and charcoal as filtering media.

1st. By the charcoal, speaking generally, a considerably larger quantity of the total residue contained in the water was removed than by the sand, their maximum results being respectively as follows :—

| Amount originally present. | Amount separated in grains in the gallon. | | Amount separated in per-centage of the quantity present. | |
|---|---|---|---|---|
|  | By sand. | By charcoal. | By sand. | By charcoal. |
| 24·578 grs. in the gallon. | 2·074 | 3·757 | 8·426 | 15·28 |

But, as might have been anticipated from the well-known

power of charcoal to remove organic principles from solution in water, its superiority over sand is more especially observable in this respect; no weighable quantity of organic matter being separated by the sand, whilst 15 per cent. of the quantity present was removed by the charcoal.

Nevertheless the case is quite altered when more impure water is filtered through the same medium; for instance, it will be remembered that sand does remove organic matter from water when the quantity present is somewhat greater.    Thus, in Tables I., II. and III. it is shown that at Chelsea, where the quantity of organic matter present in the water rose to 2·375 grs. in the gallon, then the sand removed 20·46 per cent ; when the quantity present amounted to 4·05 grs., 66·66 per cent. ; and lastly, when to 9·909 grs., no less than 95·23 per cent. was removed !

Now this is a most striking and important result in a hygienic point of view.    The evil consequences said to result from the water supply of London at present in use are mainly attributed to the organic matter present in the water; but these analyses show that the more impure the water (within certain limits), the greater is the power of porous filtering media to remove these impurities, probably from the more perfect contact between the filtering medium and the impurities present.    It is obvious that this would not go on *ad infinitum*; it must not be supposed that I wish it to be inferred that the more organic matter there is present in a water, the more would necessarily be removed by sand filtration; quite the contrary, it is highly probable that if the quantity had increased much beyond what was contained at the time of the performance of these experiments, a considerable amount might have passed through unrestrained; all I wish to be understood is, that *within the limits of the quantities present* when these experiments were made, *sand was found incapable* of removing any appreciable quantity of organic matter when the amount originally present in the water was *exceedingly small*; but that in the same ratio as the quantity present increased, the *proportion separated rose.*

Now the power of charcoal, I believe, increases in a similar manner; but no experiments could conveniently be made on the large scale at Chelsea to enable me to draw a strict comparison between the effects of sand and charcoal upon the more impure water there, similar to what had been made at Kingston with the comparatively pure water ; therefore to enable me to adduce experimental evidence on this point, a comparative experiment was made on the small scale with an artificially prepared impure water.

This water was prepared as follows :—To the sample of Thames

water collected at Kingston on the 8th of October, 1855, cow-
dung and garden mould were added, in the proportion of 4 oz. of
solid cow-dung (without straw) and 4 oz. of garden mould to 2
gallons of the water; the whole was allowed to digest for a week,
being frequently shaken during this period; it was then allowed
to settle for twelve hours, and the supernatant fetid fluid poured
off and submitted to examination. It was found to contain in
the gallon—

| | | | |
|---|---|---|---|
| Total residue | . . . | 71·772 grs. |
| Organic matter | . . | 29·308 ... |
| Mineral matter | . . | 42·468 ... |

And the results obtained by filtration respectively through sand
and charcoal are contained in the following Tables:—

TABLE VII.—Action of Vegetable Charcoal on artificially
impure Water.

| | Original con- stituents. | Present after filtration. | Amount sepa- rated in grains in the gallon. | Per-centage ratio of matter separated. |
|---|---|---|---|---|
| Total residue ...... | 71·772 | 33·86 | 37·912 | 52·82 |
| Organic matter ... | 29·308 | 3·50 | 25·808 | 88·05 |
| Mineral salts ...... | 42·464 | 30·36 | 12·104 | 28·26 |

TABLE VIII.—Action of Sand.

| | Original con- stituents. | Present after filtration. | Amount sepa- rated in grains in the gallon. | Per-centage ratio of matter separated. |
|---|---|---|---|---|
| Total residue ...... | 71·772 | 70·23 | 1·542 | 2·14 |
| Organic matter ... | 29·308 | 27·85 | 1·458 | 4·97 |
| Mineral salts ...... | 42·464 | 42·38 | 0·084 | 0·197 |

It must be borne in mind that these two experiments, being
made only on the small scale in the laboratory, though strictly
comparable with each other, do not admit of comparison with any
of the others; they serve only to demonstrate how far greater is
the power possessed by charcoal of removing organic matters
from solution than that of sand, though the difference in their
action on soluble inorganic salts is less marked.

Another point of distinction between the effects of charcoal
and sand upon the Thames water at Kingston was this—that
whereas the efficacy of the sand-filter continued increasing even
up to the conclusion of the experiment, the amount of impurity
separated by the charcoal arrived in all cases at a maximum after
a certain number of hours, and then began to decrease, as will
be more perspicuously shown by the following Table:—

TABLE IX.—Showing the period of maximum efficacy of the several filters.

| Amounts of total residue originally present in each case. | Number of hours' action of the filter. | Amount separated in grains in the gallon by | | | Per-centage ratio of quantity separated by | | |
|---|---|---|---|---|---|---|---|
| | | Sand. | Charcoal, in granules. | Charcoal, in lumps. | Sand. | Charcoal, in granules. | Charcoal, in lumps. |
| grs. | hrs. | | | | | | |
| 24·578 | 23 | 0·708 | 2·868 | ...... | 2·88 | 11·66 | |
| 23·949 | 67 | ...... | ...... | 0·281 | ...... | ...... | 1·17 |
| 23·949 | 91 | ...... | ...... | 1·873 | ...... | ...... | 7·82 |
| 23·949 | 115 | ...... | ...... | 0·937 | ...... | ...... | 3·91 |
| 24·578 | 120 | 0·888 | 2·934 | ...... | 3·613 | 11·93 | |
| 23 949 | 139 | ...... | ...... | 0·538 | ...... | ...... | 2·242 |
| 24·578 | 240 | 2·044 | 3·757 | ...... | 8·316 | 15·28 | |
| 24·578 | 376 | 2·071 | 3·204 | ...... | 8·426 | 13·03 | |

Lastly, these experiments upon the comparatively pure water of Kingston show that CHARCOAL, *as well as sand,* is capable of separating *mineral* salts from *solution* in water, as is evident by an inspection of the foregoing Tables.

The following is a brief summary of the most important results of this investigation :—It has been shown,—

1st. That sand, charcoal, and probably other porous media, possess the very peculiar property of removing, not merely suspended impurities, but even dissolved salts from solution in water.

2nd. That charcoal enjoys pre-eminently the power of abstracting organic matter from solution : but that even sand likewise is capable of effecting the same result, though to a far less extent.

3rd. That these powers, possessed by both these media, increase in intensity to a certain extent with the degree of impurity of the solution.

4th. That these properties of porous media have important bearings upon hygienic science, agricultural principles, and geological phænomena.

The reader will not fail to have noticed, by the various analyses given above, great variations in the composition of the river-water, between the two points at which experiments were made, viz. at Chelsea and Kingston, as well as at the different seasons of the year. To a number of analyses made during the year since May 1855, I hope to draw attention in a subsequent paper as elucidating these facts.

## IV. *Comparative View of the Cleavage of Crystals and Slate Rocks.* By JOHN TYNDALL, *Esq., F.R.S. &c.**

WHEN the student of physical science has to investigate the character of any natural force, his first care must be to purify it from the mixture of other forces, and thus study its simple action. If, for example, he wishes to know how a mass of water would shape itself, supposing it to be at liberty to follow the bent of its own molecular forces, he must see that these forces have free and undisturbed exercise. We might perhaps refer him to the dew-drop for a solution of the question; but here we have to do, not only with the action of the molecules of the liquid upon each other, but also with the action of gravity upon the mass, which pulls the drop downwards and elongates it. If he would examine the problem in its purity, he must do as Plateau has done, withdraw the liquid mass from the action of gravity, and he would then find the shape of the mass to be perfectly spherical. Natural processes come to us in a mixed manner, and to the uninstructed mind are a mass of unintelligible confusion. Suppose half-a-dozen of the best musical performers to be placed in the same room, each playing his own instrument to perfection: though each individual instrument might be a wellspring of melody, still the mixture of all would produce mere noise. Thus it is with the processes of nature. In nature mechanical and molecular laws mingle and create apparent confusion. Their mixture constitutes what may be called the *noise* of natural laws, and it is the vocation of the man of science to resolve this noise into its components, and thus to detect the " music " in which the foundations of nature are laid.

The necessity of this detachment of one force from all other forces is nowhere more strikingly exhibited than in the phænomena of crystallization. I have here a solution of sulphate of soda. Prolonging the mental vision beyond the boundaries of sense, we see the atoms of that liquid, like squadrons under the eye of an experienced general, arranging themselves into battalions, gathering round a central standard, and forming themselves into solid masses, which after a time assume the visible shape of the crystal which I here hold in my hand. I may, like an ignorant meddler wishing to hasten matters, introduce confusion into this order. I do so by plunging this glass rod into the vessel; the consequent action is not the pure expression of the crystalline forces; the atoms rush together with the confusion of an unor-

---

ganized mob, and not with the steady accuracy of a disciplined host. Here also in this mass of bismuth we have an example of this confused crystallization ; but in the crucible behind me a slower process is going on : here there is an architect at work "who makes no chips, no din," and who is now building the particles into crystals, similar in shape and structure to those beautiful masses which we see upon the table. By permitting alum to crystallize in this slow way, we obtain these perfect octahedrons ; by allowing carbonate of lime to crystallize, nature produces these beautiful rhomboids ; when silica crystallizes, we have formed these hexagonal prisms capped at the ends by pyramids ; by allowing saltpetre to crystallize we have these prismatic masses, and when carbon crystallizes, we have the diamond. If we wish to obtain a perfect crystal, we must allow the molecular forces free play : if the crystallizing mass be permitted to rest upon a surface it will be flattened, and to prevent this a small crystal must be so suspended as to be surrounded on all sides by the liquid, or, if it rest upon the surface, it must be turned daily so as to present all its faces in succession to the working builder. In this way the scientific man nurses these children of his intellect, watches over them with a care worthy of imitation, keeps all influences away which might possibly invade the strict morality of crystalline laws, and finally sees them developed into forms of symmetry and beauty which richly reward the care bestowed upon them*.

In building up crystals these little atomic bricks often arrange themselves into layers which are perfectly parallel to each other, and which can be separated by mechanical means ; this is called the cleavage of the crystal. I have here a crystallized mass which has thus far escaped the abrading and disintegrating forces which sooner or later determine the fate of sugar-candy. If I am skilful enough I shall discover that this crystal of sugar cleaves with peculiar facility in one direction. Here again I have a mass of rock-salt : I lay my knife upon it and with a blow cleave it in this direction ; but I find on further examining this substance that it cleaves in more directions than one. Laying my knife at right angles to its former position, the crystal cleaves again ; and finally placing the knife at right angles to the two former positions, the mass cleaves again. Thus rock-salt cleaves in three directions, and the resulting solid is this perfect cube, which may be broken up into any number of smaller cubes. Here is a mass of Iceland spar, which also cleaves in three directions, not at right angles, but oblique to each other,

* To Mr. Pattinson, of the Felling Chemical Works, Newcastle-upon-Tyne, I am indebted for some fine specimens of crystallized alum and carbonate of soda.

the resulting solid being a rhomboid. In each of these cases the mass cleaves with equal facility in all three directions. For the sake of completeness I may say that many substances cleave with unequal facility in different directions, and the heavy spar I hold in my hand presents an example of this kind of cleavage.

Turn we now to the consideration of some other phænomena to which the term cleavage may be applied. This piece of beech-wood cleaves with facility parallel to the fibre, and if our experiments were fine enough we should discover that the cleavage is most perfect when the edge of the axe is laid across the rings which mark the growth of the tree. The fibres of the wood lie side by side, and a comparatively small force is sufficient to separate them. If you look at this mass of hay severed from a rick, you will see a sort of cleavage developed in it also; the stalks lie in parallel planes, and only a small force is required to separate them laterally. But we cannot regard the cleavage of the tree as the same in character as the cleavage of the hayrick. In the one case it is the atoms arranging themselves according to organic laws which produce a cleavable structure, in the other case the easy separation in a certain direction is due to the mechanical arrangement of the coarse sensible masses of the stalks of hay.

In like manner I find that this piece of sandstone cleaves parallel to the planes of bedding. This rock was once a powder, more or less coarse, held in mechanical suspension by water. The powder was composed of two distinct parts, fine grains of sand and small plates of mica. Imagine a wide strand covered by a tide which holds such powder in suspension*: how will it sink? The rounded grains of sand will reach the bottom first, the mica afterwards, and when the tide recedes we have the little plates shining like spangles upon the surface of the sand. Each successive tide brings its charge of mixed powder, deposits its duplex layer day after day, and finally masses of immense thickness are thus piled up, which by preserving the alternations of sand and mica tell the tale of their formation. I do not wish you to accept this without proof. Take the sand and mica, mix them together in water, and allow them to subside, they will arrange themselves in the manner I have indicated; and by repeating the process you can actually build up a sandstone mass which shall be the exact counterpart of that presented by nature, as I have done in this glass jar. Now this structure cleaves with readiness along the planes in which the particles of mica are strewn. Here is a mass of such a rock sent to me from

---

* I merely use this as an illustration; the deposition may have really been due to sediment carried down by rivers. But the action must have been periodic, and the powder duplex.

Halifax : here are other masses from the quarries of Over Darwen in Lancashire\*. With a hammer and chisel you see I can cleave them into flags ; indeed these flags are made use of for roofing purposes in the districts from which the specimens have come, and receive the name of "slatestone." But you will discern without a word from me, that this cleavage is not a crystalline cleavage any more than that of a hayrick is. It is not an arrangement produced by molecular forces ; indeed it would be just as reasonable to suppose that on this jar of sand and mica the particles arranged themselves into layers by the forces of crystallization, instead of by the simple force of gravity, as to imagine that such a cleavage as this could be the product of crystallization.

This, so far as I am aware of, has never been imagined, and it has been agreed among geologists not to call such splitting as this cleavage at all, but to restrict the term to a class of phænomena which I shall now proceed to consider.

Those who have visited the slate quarries of Cumberland and North Wales will have witnessed the phænomena to which I refer. We have long drawn our supply of roofing-slates from such quarries ; schoolboys ciphered on these slates, they were used for tombstones in churchyards, and for billiard-tables in the metropolis ; but not until a comparatively late period did men begin to inquire how their wonderful structure was produced. What is the agency which enables us to split Honister Crag, or the cliffs of Snowdon, into laminæ from crown to base ? This question is at the present moment one of the greatest difficulties of geologists, and occupies their attention perhaps more than any other. You may wonder at this. Looking into the quarry of Penrhyn, you may be disposed to explain the question as I heard it explained two years ago. "These planes of cleavage," said a friend who stood beside me on the quarry's edge, "are the planes of stratification which have been lifted by some convulsion into an almost vertical position." But this was a great mistake, and indeed here lies the grand difficulty of the problem. These planes of cleavage stand in most cases at a high angle to the bedding. Thanks to Sir Roderick Murchison, who has kindly permitted me the use of specimens from the Museum of Practical Geology (and here I may be permitted to express my acknowledgements to the distinguished staff of that noble establishment, who, instead of considering me an intruder, have welcomed me as a brother), I am able to place the proof of this before you. Here is a mass of slate in which the planes of bedding are distinctly marked ; here are the

\* For the specimens from Halifax I have to thank Mr. Richard Carter, and for those from Darwen I am indebted to Mr. J. Singleton.

planes of cleavage, and you see that one of them makes a large angle with the other. The cleavage of slates is therefore not a question of stratification, and the problem which we have now to consider is, " By what cause has this cleavage been produced ?"

In an able and elaborate essay on this subject in 1835, Prof. Sedgwick proposed the theory that cleavage is produced by the action of crystalline or polar forces after the mass has been consolidated. " We may affirm," he says, " that no retreat of the parts, no contraction of dimensions in passing to a solid state can explain such phænomena. They appear to me only resolvable on the supposition that crystalline or polar forces acted upon the whole mass simultaneously in one direction and with adequate force." And again, in another place : " Crystalline forces have rearranged whole mountain masses, producing a beautiful crystalline cleavage, passing alike through all the strata\*." The utterance of such a man struck deep, as was natural, into the minds of geologists, and at the present day there are few who do not entertain this view either in whole or in part†. The magnificence of the theory, indeed, has, in some cases, caused speculation to run riot, and we have books published, aye and largely sold, on the action of polar forces and geologic magnetism, which rather astonish those who know something about the subject. According to the theory referred to, miles and miles of the districts of North Wales and Cumberland, comprising huge mountain masses, are neither more nor less than the parts of a gigantic crystal. These masses of slate were originally fine mud ; this mud is composed of the broken and abraded particles of older rocks. It contains silica, alumina, iron, potash, soda, and mica mixed in sensible masses mechanically together. In the course of ages the mass became consolidated, and the theory before us assumes that afterwards a process of crystallization rearranged the particles and developed in the mass a single plane of crystalline cleavage. With reference to this hypothesis, I will only say that it is a bold stretch of analogies ; but still it has done good service : it has drawn atten-

\* Transactions of the Geological Society, ser. ii. vol. iii. p. 477.
† In a letter from Sir Charles Lyell, dated from the Cape of Good Hope, February 20, 1836, Sir John Herschel writes as follows :—" If rocks have been so heated as to allow of a commencement of crystallization, that is to say, if they have been heated to a point at which the particles can begin to move amongst themselves, or at least on their own axes, some general law must then determine the position in which these particles will rest on cooling. Probably that position will have some relation to the direction in which the heat escapes. Now when all or a majority of particles of the same nature have a general tendency to one position, that must of course determine a cleavage plane."

tion to the question; right or wrong a theory thus thoughtfully uttered has its value; it is a dynamic power which operates against intellectual stagnation; and even by provoking opposition is eventually of service to the cause of truth. It would however have been remarkable if, among the ranks of geologists themselves, men were not found to seek an explanation of the phænomena in question, which involved a less hardy spring on the part of the speculative faculty than the view to which I have just referred.

The first step in an inquiry of this kind is to put oneself into contact with nature, to seek facts. This has been done, and the labours of Sharpe (the late President of the Geological Society, who, to the loss of science and the sorrow of all who knew him, has so suddenly been taken away from us), Sorby and others have furnished us with a body of evidence which reveals to us certain important physical phænomena, associated with the appearance of slaty cleavage, if they have not produced it: the nature of this evidence we will now proceed to consider.

Fossil shells are found in these slate-rocks. I have here several specimens of such shells, occupying various positions with regard to the cleavage planes. They are squeezed, distorted and crushed. In some cases a flattening of the convex shell occurs, in others the valves are pressed by a force which acted in the plane of their junction, but in all cases the distortion is such as leads to the inference that the rock which contains these shells has been subjected to enormous pressure in a direction at right angles to the planes of cleavage; the shells are all flattened and spread out upon these planes. I hold in my hand a fossil trilobite of normal proportions. Here is a series of fossils of the same creature which have suffered distortion. Some have lain across, some along, and some oblique to the cleavage of the slate in which they are found; in all cases the nature of the distortion is such as required for its production a compressing force acting at right angles to the planes of cleavage. As the creatures lay in the mud in the manner indicated, the jaws of a gigantic vice appear to have closed upon them and squeezed them into the shape you see. As further evidence of the exertion of pressure, let me introduce to your notice a case of contortion which has been adduced by Mr. Sorby. The bedding of the rock shown in this figure was once horizontal; at A we have a deep layer of mud, and at *mn* a layer of comparatively unyielding gritty material; below that again, at B, we have another layer of the fine mud of which slates are formed. This mass cleaves along the shading lines of the diagram: but look at the shape of the intermediate bed: it is contorted into a serpentine form, and leads irresistibly to the conclu-

sion that the mass has been pressed together at right angles to the planes of cleavage. This action can be experimentally imitated, and I have here a piece of clay in which this is done and the same result produced on a small scale. The amount of compression, indeed, might be roughly estimated by supposing this contorted bed *m n* to be stretched out, its length measured and compared with the distance *c d*; we find in this way that the yielding of the mass has been considerable.

Let me now direct your attention to another proof of pressure; you see the varying colours which indicate the bedding on this mass of slate. The dark portion, as I have stated, is gritty, and composed of comparatively coarse particles, which, owing to their size, shape and gravity, sink first and constitute the bottom of each layer. Gradually, from bottom to top the coarseness di-

minishes, and near the upper surface of each layer we have a mass of comparatively fine clean mud. Sometimes this fine mud forms distinct layers in a mass of slate-rock, and it is the mud thus consolidated from which are derived the German razor-stones, so much prized for the sharpening of surgical instruments. I have here an example of such a stone; when a bed is thin, the clean white mud is permitted to rest, as in this case, upon a slab of the coarser slate in contact with it : when the bed is thick, it is cut into slices which are cemented to pieces of ordinary slate, and thus rendered stronger. The mud thus deposited sometimes in layers is, as might be expected, often rolled up into nodular masses, carried forward, and deposited by the rivers from which the slate-mud has subsided. Here, indeed, are such nodules enclosed in sandstone. Everybody who has ciphered upon a school-slate must remember the whitish-green spots which sometimes dotted the surface of the slate; he will remember how his slate-pencil usually slid over such spots as if they were greasy ; now these spots are composed of the finer mud, and they could not, on account of their fineness, *bite* the pencil like
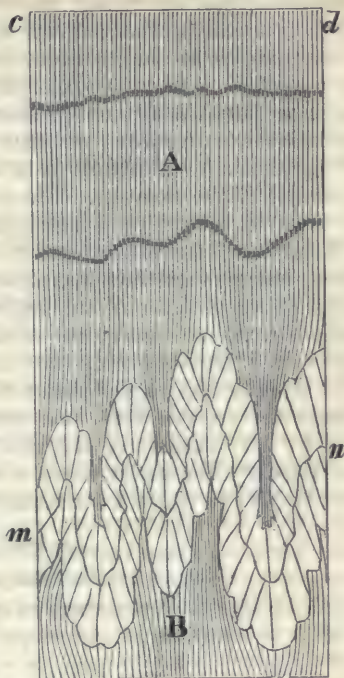
the surrounding gritty portions of the slate.   Here is a beauti-
ful example of the spots : you observe them on the cleavage sur-
face in broad patches ; but if this mass has been compressed at
right angles to the planes of cleavage, ought we to expect the
same marks when we look at the edge of the slab ?   The nodules
will be flattened by such pressure, and we ought to see evidence
of this flattening when we turn the slate edgeways.   Here it is.
The section of a nodule is a sharp ellipse with its major axis
parallel to the cleavage.   There are other examples of the same
nature on the table ; I have made excursions to the quarries of
Wales and Cumberland, and to many of the slate-yards of Lon-
don, but the same fact invariably appears, and thus we elevate a
common experience of our boyhood into evidence of the highest
significance as regards one of the most important problems of
geology.   In examining the magnetism of these slates, I was
led to infer that these spots would contain a less amount of iron
than the surrounding dark slate.   The analysis was made for
me by Mr. Hambly in the laboratory of Dr. Percy at the School
of Mines.   The result which is stated in this Table, justifies the
conclusion to which I have referred.

*Analysis of Slate.*

Purple Slate, two analyses.

1. Per-centage of iron   .   .   .   5·85
2.        „        „        .   .   .   6·13
                    Mean   .   .   5·99

Greenish Slate.

1. Per-centage of iron   .   .   .   .   3·24
2.        „        „        .   .   .   3·12
                    Mean   .   .   3·18

The quantity of iron in the dark slate immediately adjacent to
the greenish spot is, according to these analyses, nearly double
of the quantity contained in the spot itself.   This is about the
proportion which the magnetic experiments suggested.

Let me now remind you that the facts which I have brought
before you are typical facts—each is the representative of a class.
We have seen shells crushed ; the unhappy trilobites squeezed,
beds contorted, nodules of greenish marl flattened ; and all these
sources of independent testimony point to one and the same
conclusion, namely, that slate-rocks have been subjected to enor-
mous pressure in a direction at right angles to the planes of
cleavage*.

* While to my mind the evidence in proof of pressure seems perfectly
irresistible, I by no means assert that the manner in which I have stated it
is incapable of modification.   All that I deem important is the fact that

In reference to Mr. Sorby's contorted bed, I have said that by supposing it to be stretched out and its length measured, it would give us an idea of the amount of yielding of the mass above and below the bed. Such a measurement, however, would not quite give the amount of yielding; and here I would beg your attention to a point, the significance of which has, so far as I am aware of, hitherto escaped attention. I hold in my hand a specimen of slate with its bedding marked upon it; the lower portions of each bed are composed of a comparatively coarse gritty material something like what you may suppose this contorted bed to be composed of. Well, I find that the cleavage takes a bend in crossing these gritty portions, and that the tendency of these portions is to cleave more at right angles to the bedding. Look to this diagram: when the forces commenced to act, this intermediate bed, which though comparatively unyielding is not entirely so, suffered longitudinal pressure; as it bent, the pressure became gradually more lateral, and the direction of its cleavage is exactly such as you would infer from a force of this kind—it is neither quite across the bed, nor yet in the same direction as the cleavage of the slate above and below it, but intermediate between both. Supposing the cleavage to be at right angles to the pressure, this is the direction which it ought to take across these more unyielding strata.

Thus we have established the concurrence of the phænomena of cleavage and pressure—that they accompany each other; but the question still remains, Is this pressure of itself sufficient to account for the cleavage? A single geologist, as far as I am aware, answers boldly in the affirmative. This geologist is Sorby, who has attacked the question in the true spirit of a physical investigator. You remember the cleavage of the flags of Halifax and Over Darwen, which is caused by the interposition of plates of mica between the layers. Mr. Sorby examines the structure of slate-rock, and finds plates of mica to be a constituent. He asks himself, what will be the effect of pressure upon a mass containing such plates confusedly mixed up in it? It will be, he argues, and he argues rightly, to place the plates with their flat surfaces more or less perpendicular to the direction in which the pressure is exerted. He takes scales of the oxide of iron, mixes them with a fine powder, and on squeezing the mass finds that the tendency of the scales is to set themselves at right angles to the line of pressure. Now the planes in which these plates arrange themselves will, he contends, be those along which the mass cleaves.

pressure has been exerted; and provided this remain firm, the fate of any minor portion of the evidence by which it is here established is of comparatively little moment.

I could show you by tests of a totally different character from those applied by Mr. Sorby, how true his conclusion is, that the effect of pressure on elongated particles, or plates, will be such as he describes it.   Nevertheless, while knowing this fact, and admiring the ability with which Mr. Sorby has treated this question, I cannot accept his explanation of slate-cleavage.   I believe that even if these plates of mica were wholly absent the cleavage of slate-rocks would be much the same as it is at present.

I will not dwell here upon minor facts,—I will not urge that the perfection of the cleavage bears no relation to the quantity of mica present; but I will come at once to a case which to my mind completely upsets the notion that such plates are a necessary element in the production of cleavage.

Here is a mass of pure white wax : there are no mica particles here ; there are no scales of iron, or anything analogous mixed up with the mass.   Here is the self-same substance submitted to pressure.   I would invite the attention of the eminent geologists whom I see before me to the structure of this mass.   No slate ever exhibited so clean a cleavage; it splits into laminæ of surpassing tenuity, and proves at a single stroke that pressure is sufficient to produce cleavage, and that this cleavage is independent of the intermixed plates of mica assumed in Mr. Sorby's theory.   I have purposely mixed this wax with elongated particles, and am unable to say at the present moment that the cleavage is sensibly affected by their presence,—if anything, I should say they rather impair its fineness and clearness than promote it.

The finer the slate the more perfect will be the resemblance of its cleavage to that of the wax.   Compare the surface of the wax with the surface of this slate from Borrodale in Cumberland. You have precisely the same features in both : you see flakes clinging to the surfaces of each, which have been partially torn away by the cleavage of the mass : I entertain the conviction that if any close observer compares these two effects, he will be led to the conclusion that they are the product of a common cause*.

But you will ask me how, according to my view, does pressure produce this remarkable result. This may be stated in a very few words.

Nature is everywhere imperfect !   The eye is not perfectly achromatic, the colours of the rose and tulip are not pure colours,

---

* I have usually softened the wax by warming it, kneaded it with the fingers, and pressed it between thick plates of glass previously wetted.   At the ordinary summer temperature the wax is soft, and tears rather than cleaves ; on this account I cool my compressed specimens in a mixture of pounded ice and salt, and when thus cooled they split beautifully.

and the freshest air of our hills has a bit of poison in it. In like manner there is no such thing in nature as a body of perfectly homogeneous structure. I break this clay which seems so intimately mixed, and find that the fracture presents to my eyes innumerable surfaces along which it has given way, and it has yielded along these surfaces because in them the cohesion of the mass is less than elsewhere. I break this marble, and even this wax, and observe the same result: look at the mud at the bottom of a dried pond; look to some of the ungravelled walks in Kensington Gardens on drying after rain,—they are cracked and split, and other circumstances being equal, they crack and split where the cohesion of the mass is least. Take then a mass of partially consolidated mud. Assuredly such a mass is divided and subdivided by surfaces along which the cohesion is comparatively small. Penetrate the mass, and you will see it composed of numberless irregular nodules bounded by surfaces of weak cohesion. Figure to your mind's eye such a mass subjected to pressure,—the mass yields and spreads out in the direction of least resistance*; the little nodules become converted into laminæ, separated from each other by surfaces of weak cohesion, and the infallible result will be that such a mass will cleave at right angles to the line in which the pressure is exerted.

Further, a mass of dried mud is full of cavities and fissures. If you break dried pipe-clay you see them in great numbers, and there are multitudes of them so small that you cannot see them. I have here a piece of glass in which a bubble was enclosed; by the compression of the glass the bubble is flattened, and the sides of the bubble approach each other so closely as to exhibit the colours of thin plates. A similar flattening of the cavities must take place in squeezed mud, and this must materially facilitate the cleavage of the mass in the direction already indicated.

Although the time at my disposal has not permitted me to develope this thought as far as I could wish, yet for the last twelve months the subject has presented itself to me almost daily under one aspect or another. I have never eaten a biscuit during this period in which an intellectual joy has not been superadded to the more sensual pleasure, for I have remarked in all such cases cleavage developed in the mass by the rolling-pin of the pastrycook or confectioner. I have only to break these cakes, and to look at the fracture, to see the laminated structure of the mass; nay, I have the means of pushing the analogy further, I have here some slate which was subjected to

---

* It is scarcely necessary to say that if the mass were squeezed equally in *all* directions no laminated structure could be produced; it must have room to yield in a lateral direction.

a high temperature during the conflagration of Mr. Scott Russell's premises. I invite you to compare this structure with that of a biscuit; air or vapour within the mass has caused it to swell, and the mechanical structure it reveals is precisely that of a biscuit. I have gone a little into the mysteries of baking while conducting my inquiries on this subject, and have received much instruction from a lady friend in the manufacture of puff-paste. Here is some such paste baked in this house under my own superintendence. The cleavage of our hills is accidental cleavage, but this is cleavage with intention. The volition of the pastrycook has entered into the formation of the mass, and it has been his aim to preserve a series of surfaces of structural weakness, along which the dough divides into layers. Puff-paste must not be handled too much, for then the continuity of the surfaces is broken; it ought to be rolled on a cold slab, to prevent the butter from melting, and diffusing itself through the mass, thus rendering it more homogeneous and less liable to split. This is the whole philosophy of puff-paste; it is a grossly exaggerated case of slaty cleavage.

As time passed on cases multiplied, illustrating the influence of pressure in producing lamination. Mr. Warren De la Rue informs me that he once wished to obtain white-lead in a fine granular state, and to accomplish this he first compressed the mass: the mould was conical, and permitted the mass to spread a little laterally under the pressure. The lamination was as perfect as that of slate, and quite defeated him in his effort to obtain a granular powder. Mr. Brodie, as you are aware, has recently discovered a new kind of graphite: here is the substance in powder, of exquisite fineness. This powder has the peculiarity of clinging together in little confederacies; it cannot be shaken asunder like lycopodium; and when the mass is squeezed, these groups of particles flatten, and a perfect cleavage is produced. Mr. Brodie himself has been kind enough to furnish me with specimens for this evening's lecture. I will cleave them before you: you see they split up into plates which are perpendicular to the line in which the pressure was exerted. This testimony is all the more valuable, as the facts were obtained without any reference whatever to the question of cleavage.

I have here a mass of that singular substance Boghead cannel*. This was once a mass of mud, more or less resembling this one, which I have obtained from a bog in Lancashire. I feel some hesitation in bringing this substance before you, for, as in other cases, so in regard to Boghead cannel, science—not science, let me not libel it, but the quibbling, litigious, money-loving portion of human nature speaking through the mask of science

---

* For which I have to thank Mr. George Edmondson.

—has so contrived to split hairs as to render the qualities of the substance somewhat mythical. I shall therefore content myself with showing you how it cleaves, and with expressing my conviction that pressure had a great share in the production of this cleavage.

The principle which I have enunciated is so simple as to be almost trivial; nevertheless, it embraces not only the cases I have mentioned, but, if time permitted, I think I could show you that it takes a much wider range. When iron is taken from the puddling furnace it is a more or less spongy mass: it is at a welding heat, and at this temperature is submitted to the process of rolling: bright smooth bars such as this are the result of this rolling. But I have said that the mass is more or less spongy or nodular, and, notwithstanding the high heat, these nodules do not perfectly incorporate with their neighbours: what then? You would say that the process of rolling must draw the nodules into fibres—it does so; and here is a mass acted upon by dilute sulphuric acid, which exhibits in a striking manner this fibrous structure. The experiment was made by my friend Dr. Percy, without any reference to the question of cleavage.

Here are other cases of fibrous iron. This fibrous structure is the result of mechanical treatment. Break a mass of ordinary iron and you have a granular fracture; beat the mass, you elongate these granules, and finally render the mass fibrous. Here are pieces of rails along which the wheels of locomotives have slided *; the granules have yielded and become plates. They exfoliate or come off in leaves; all these effects belong, I believe, to the great class of phænomena of which slaty cleavage forms the most prominent example †.

Thus, ladies and gentlemen, we have reached the termination of our task. I commenced by exhibiting to you some of the phænomena of crystallization. I have placed before you the facts which are found to be associated with the cleavage of slate rocks. These facts, as finely expressed by Helmholtz, are so many telescopes to our spiritual vision, by which we can see backward through the night of antiquity, and discern the forces which have been in operation upon the earth's surface

> " Ere the lion roared,
> Or the eagle soared."

From evidence of the most independent and trustworthy

* For these specimens and other valuable assistance I am indebted to Mr. Williams.

† An eminent authority informs me that he believes these surfaces of weak cohesion to be due to the interposition of films of graphite, and not to any tendency of the iron itself to become fibrous: this of course does not in any way militate against the theory which I have ventured to propose. All that the theory requires is surfaces of weak cohesion, however produced, and a change of shape of such surfaces consequent on pressure or rolling.

character, we come to the conclusion that these slaty masses have been subjected to enormous pressure, and by the sure method of experiment we have shown—and this is the only really new point which has been brought before you—how the pressure is sufficient to produce the cleavage. Expanding our field of view, we find the self-same law, whose footsteps we trace amid the crags of Wales and Cumberland, stretching its ubiquitous fingers into the domain of the pastrycook and ironfounder; nay, a wheel cannot roll over the half-dried mud of our streets without revealing to us more or less of the features of this law. I would say, in conclusion, that the spirit in which this problem has been attacked by geologists, indicates the dawning of a new day for their science. The great intellects who have laboured at geology, and who have raised it to its present pitch of grandeur, were compelled to deal with the subject in mass; they had no time to look after details. But the desire for more exact knowledge is increasing; facts are flowing in, which, while they leave untouched the intrinsic wonders of geology, are gradually supplanting by solid truths the uncertain speculations which beset the subject in its infancy. Geologists now aim to imitate, as far as possible, the conditions of nature, and to produce her results; they are approaching more and more to the domain of physics, and I trust the day will soon come when we shall interlace our friendly arms across the common boundary of our sciences, and pursue our respective tasks in a spirit of mutual helpfulness, encouragement and goodwill.

V. *On the Form of an Alloy of Bismuth.   By* W. H. MILLER, *M.A., F.R.S., F.G.S., Professor of Mineralogy in the University of Cambridge; with an Analysis of the Alloy, by* ALLAN DICK, *Esq., Metallurgical Laboratory, School of Mines\*.*

THE specimen was obtained by Dr. Percy from Mr. Pershouse of Birmingham.

The crystals belong to the prismatic system. The symbols of their simple forms are—c, 001; r, 111.

The angles between normals to their faces are,—



| | ° | ′ |
|----|----|----|
| rc | 60 | 17 |
| rr′ | 61 | 38 |
| rr″ | 89 | 4 |

---
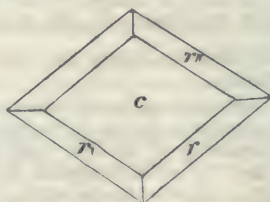
\* Communicated by the Author.

The plane angles of the face $c$ are $72° 17'$ and $107° 43'$.
Twins. Twin-face $c$.



| | |
|---|---|
| $c$ | $59° 26'$ |
| $r\iota$ | $1\ 52$ |
| $\iota r''$ | $90\ 56$ |

Cleavage. $c$, very perfect and easily obtained.

### Analysis.

Specific gravity $= 9.46$.

A well-crystallized portion of it was reduced to powder, examined in the usual manner, and found to consist of sulphur, bismuth, copper, and nickel.

*Estimation of Sulphur.*—10·43 grs. were digested in warm nitric acid, till the sulphur which separated had melted into a globule. Weight of sulphur globule 0·19 gr. It was roasted, and the residue weighed 0·04 gr. This was dissolved in nitric acid and added to the original solution, from which the sulphuric acid was precipitated and weighed as sulphate of baryta. Weight of sulphate of baryta obtained 1·05 gr.

*Estimation of the Metals.*—10·43 grs. were dissolved as above, the solution diluted, and a current of sulphuretted hydrogen passed through it. The sulphurets were collected on a filter, and washed with acidulated water containing sulphuretted hydrogen. The nickel was precipitated as oxide by caustic potash, reduced by hydrogen, and weighed as metal 0·59 gr.

The sulphurets were dissolved in nitric acid, and the bismuth separated from the copper by means of carbonate of ammonia. The treatment with carbonate of ammonia was repeated in order to remove the last traces of copper from the precipitated bismuth, which was ignited and weighed 10·27 grs.

The ammoniacal filtrate was evaporated to dryness and the ammoniacal salts expelled. The residue was dissolved in nitric acid, and the copper precipitated as oxide by caustic potash. Weight of oxide of copper 0·30 gr.

### Results tabulated.

| | | | | | |
|---|---|---|---|---|---|
| Bismuth | . | . | . | . | 88·49 |
| Nickel | . | . | . | . | 5·65 |
| Copper | . | . | . | . | 2·29 |
| Sulphur | . | . | . | . | 2·82 |
| | | | | | 99·25 |

Estimating the sulphur in combination with the copper and a portion of the bismuth, and considering these sulphides as foreign matter, the formula will be very nearly that of $Ni\,Bi^2$, the atomic weight of bismuth being taken as 208.

VI. *On the Density of the Earth, deduced from the Experiments of the* Astronomer Royal, *in Harton Coal-pit. By the* Rev. Samuel Haughton, *M.A., Fellow of Trinity College, Dublin\*.*

SOME time ago the following density of the earth was deduced by me, by the aid of very simple considerations, from the pendulum observations of the Astronomer Royal in Harton Coal-pit, South Shields, as reported in the newspapers.

The method of deducing the density is rough, and cannot be considered as having the same value as the more precise and detailed considerations on which Mr. Airy has based his own calculations; yet as it is simple, and easily understood, it may prove of some interest to the readers of the Philosophical Magazine. It is based on the following facts:—

1st. From the fact that the seconds' pendulum gains $2\frac{1}{4}$ seconds per day at the bottom of the coal-pit, I infer that gravity at the surface is to gravity at the bottom of the pit as 19200 to 19201.

2nd. If a spheroidal shell be drawn through the bottom of the coal-pit similar to the spheroid forming the surface of the sea, the mass of the whole earth is equal to the mass of this spheroid, plus the matter exterior to it, including sea and land.

The two foregoing propositions, being facts, will easily be admitted; I now add to them an hypothesis which very probably does not deviate far from the truth; it is as follows:—

*That the attraction of the sea and land, external to the spheroid through the bottom of the coal-pit, does not differ sensibly from the attraction of a shell bounded externally by a similar surface, and having a mean density the same as the mean density of the sea and land external to the spheroid.*

If this be admitted, we can state the two facts already given as follows:—

$$\delta = \frac{19201}{19200}\left(1 + \frac{h}{r}\right)\Delta, \quad \ldots \ldots \quad (1)$$

$$\Delta = \left(1 + \frac{3h}{r}\right)\delta + \frac{3h}{r}\rho. \quad \ldots \ldots \quad (2)$$

In these equations—

$\Delta$ is the mean density of the whole earth;

$\delta$ is the mean density of the portion included by the spheroid passing through the bottom of the coal-pit;

$\rho$ is the mean density of the land and water above the bottom of the coal-pit;

$\frac{h}{r}$ is the ratio of the depth of the coal-pit to the radius of the earth.

* Communicated by the Author.

Assuming the depth of the pit to be 1260 feet, and the radius of the earth 4000 miles, we have $\dfrac{h}{r} = \dfrac{1}{16762}$. Eliminating $\delta$ from equations (1) and (2), and substituting this value of $\dfrac{h}{r}$, we find

$$\Delta = \frac{1440}{541}\rho. \qquad \qquad \text{(3)}$$

To find the value of $\rho$, I make use of the following facts:—According to Humboldt, the mean height of the continents above the sea-level is 1000 feet; and according to Rigaud, the relative areas of land and water are as 1 to 2·815. Hence, since the bottom of the coal-pit is 1200 feet below the sea-level, if we suppose all the land and water blended into one couche of a thickness of 1200 feet, and denote its mean density by $\rho$, we find, *assuming the mean density of the land to be* 2·75,

$$2200 \times l \times 2{\cdot}75 + 1200 \times w \times 1 = 1200(w+l)\rho;$$

$w$ denoting the area of water, and $l$ the area of land; and substituting for these their values, we obtain finally

$$\rho = 2{\cdot}059. \qquad \qquad \text{(4)}$$

Substituting this mean density of the land and water at the surface for $\rho$ in equation (3), we find

$$\Delta = 5{\cdot}480. \qquad \qquad \text{(5)}$$

This result agrees closely with the best experiments made with the torsion balance, and appears to be fairly deducible from the observed facts, considered in a general point of view.

Trinity College, Dublin,
    June 18, 1856.

---

## VII. *Chemical Notices from Foreign Journals.*
### *By* E. Atkinson, *Ph.D.*

[Continued from vol. xi. p. 461.]

IN the April Number of the *Annales de Chimie et de Physique,* Berthelot has an important paper on the relation between Formic Acid and Carbonic Oxide.

Carbonic oxide bears the same relation to formic acid that olefiant gas does to alcohol:—

$$C^2O^2 \;+\; 2HO \;=\; C^2H^2O^4,$$
<div align="center">Carbonic oxide.        Formic acid.</div>

$$C^4H^4 \;+\; 2HO \;=\; C^4H^6O^2;$$
<div align="center">Olefiant gas.        Alcohol.</div>
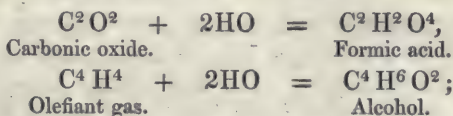
and formic acid, heated with strong sulphuric acid, gives carbonic oxide; just as alcohol, by the same treatment, gives olefiant gas.

On this relation Berthelot founds a new method for the formation of formic acid, which consists in placing carbonic oxide under conditions favourable to the fixation of the elements of water. By enclosing carbonic oxide along with moist potash in a flask, and heating for several days, this condition is satisfied: the carbonic oxide is perfectly absorbed by the potash, which is converted into formiate of potash, from which formic acid is obtained by appropriate methods.

In the hope of uniting directly nascent carbonic oxide with the elements of water, Berthelot investigated the decomposition of oxalic acid at 100° C. under different circumstances; heated alone, it is, to a slight extent, decomposed into carbonic acid and carbonic oxide. This decomposition is much accelerated by the presence of acids, and is not due to a chemical but to a catalytic action. When oxalic acid is heated to 100° C. with syrupy glycerine, a copious evolution of pure carbonic acid takes place. At the expiration of some time this evolution ceases, and the glycerine then contains half the carbon of the oxalic acid in the form of formic acid, which is not in combination with the glycerine, but merely dissolved like ammoniacal gas in water.

Here the carbonic oxide and the water produced simultaneously and in the nascent state from the decomposition of the oxalic acid unite to produce formic acid, the glycerine exercising merely a catalytic action.

By repeatedly adding water to the glycerine and distilling, the whole of the formic acid contained in it passes over. This process is extremely productive, as nearly the theoretical quantity is obtained, and the glycerine may be used over and over again, for it is not acted on.

By this means also pure carbonic oxide may be obtained; for if in the preparation of formic acid, after the evolution of carbonic acid has ceased, the glycerine be heated to 200°, the whole of the formic acid contained in it is decomposed into water and carbonic oxide, which passes off perfectly pure.

When carbonic oxide is passed into a saturated solution of protochloride of copper in hydrochloric acid, a compound of the two bodies is obtained which crystallizes in pearly laminæ, and has the formula

$$4Cu^2 Cl, 3CO 7HO.$$

Berthelot found that hydrobromic acid, when left in contact with mercury for a very long time, is decomposed into bromide of mercury and hydrogen. The same change takes place, but much more rapidly, when these bodies are heated together to 100° C. This slow decomposition of hydrobromic acid by mercury is a new example of the influence of time on chemical action.

Boussingault communicates some experiments on the use of arsenic in steeping grain for seed. This process has two objects, the one to protect the harvest from disease, the other to prevent the seed from being devoured by vermin. The substances generally used are salt, glauber salt, lime, and sulphate of copper. But although these may hinder the development of cryptogamic sporules, they have little effect in preventing the seed from being eaten. The greatest part of the substance used remains in the husk, which the animal rejects.

The most effectual means is the employment of arsenic; this not only preserves the seed from decay, but if eaten by the vermin it destroys them, being so strongly poisonous. By using arsenic in a soluble form, such as the arsenite of soda, it may be added to the grain in perfectly definite proportions.

Boussingault's process is as follows:—A solution of arsenite of soda is prepared, which contains 57 grammes of arsenious acid in the litre. Of this arsenical solution $3\frac{1}{2}$ litres are taken and added to $12\frac{1}{2}$ litres of water. A hectolitre of corn is placed in a large tub, and these 16 litres of mixture are added, the corn being continually stirred. In about an hour the whole of the liquid is absorbed, and the grain is then dried. It is, of course, necessary to exercise extreme care in using the arsenical solution, and it is well to colour it strongly by the addition of sulphate of iron and prussiate of potash, so that its presence would be readily betrayed.

This steeping is not an unprofitable affair, for it first effectually preserves the harvest, and secondly, by killing the vermin which might devour it, converts them into useful manure.

An important paper by Fremy on the fluorides is published in the May Number of the same journal. The results he has arrived at are best seen from his own summary of them.

1. Hydrofluoric acid may be obtained anhydrous and pure, by calcining in an apparatus of platinum hydrofluate of fluoride of potassium previously well dried. In this state the acid is gaseous at ordinary temperatures; it attacks glass and all silicious substances energetically, contrary to what has been asserted on the subject in the last few years.

2. All the experiments made, confirm the constitution of this acid as admitted by most chemists, viz. that it is a hydrogen acid.

3. The fluorides may be divided into three classes, each of which is characterized by the possession of important properties. The first class contains the anhydrous fluorides, and is analogous to the chlorides; the second class, the hydrated fluorides, which comport themselves like the hydrochlorates; in the third class are found the hydrofluates of the fluorides, which are true acid salts.

4. The anhydrous fluorides are remarkable for their stability; the hydrated fluorides, on the contrary, are only slightly stable, and are decomposed even by being dried *in vacuo*, hydrofluoric acid being disengaged, and a residue of oxyfluoride or oxide being left.

5. The anhydrous fluorides have a strong tendency to form double fluorides; this property is seen even with insoluble fluorides. These last compounds cannot be obtained by double decomposition, as they always retain, as a double salt, part of the soluble salt used in their preparation.

6. Hydrogen does not decompose all the fluorides, even at a high temperature; it does not act on fluoride of calcium, but it reduces the fluorides of lead, of tin, &c. with the greatest ease. The reduction by hydrogen of metallic fluorides such as those of lead, tin, &c., which resist the reducing action of carbon, appears to demonstrate in a positive manner that these compounds contain no oxygen, and are really binary compounds.

7. All the fluorides, even those of potassium, sodium, and calcium, are rapidly decomposed by the vapour of water.

8. Oxygen and chlorine decompose fluoride of calcium at furnace heat, and liberate a gas which appears to be fluorine.

9. Vapour of sulphur does not act on fluoride of calcium, but this body is completely decomposed by the vapour of sulphide of carbon; there is formed sulphide of calcium, and probably fluoride of carbon: the presence of silicious substances facilitates the reaction.

10. The analyses of the principal fluorides made, such as those of potassium, sodium, calcium, tin, lead, and silver, show that the equivalent of fluorine, as determined by Berzelius, is correct.

11. All the fusible fluorides are decomposed by the voltaic current, and disengage a gas which appears to be the radical of the fluorides.

The author is still engaged on the subject, and promises still more extensive contributions.

In the preparation of aluminium, Deville often accidentally obtained a dark gray crystalline substance, which was found to be silicon in a form analogous to that of graphite. Wöhler communicates, in Liebig's *Annalen* for March, a method by which this allotropic modification of silicon may be produced at pleasure.

Aluminium is fused in a Hessian crucible with about twenty times its weight of perfectly dry silico-fluoride of potassium. On cooling, the slag in the crucible is found to contain a well-formed dark-coloured regulus, from which, by treatment with hydrochloric acid, a crystalline mass is obtained, consisting of about 70 per cent. of the aluminium used. In this case the aluminium appears to determine the crystallization of the silicon, as melting cast iron determines that of graphite.

The silicon in this condition forms large and small opake crystalline laminæ possessing a metallic lustre, and very similar in appearance to natural and artificial graphite; but it has more of a metallic lustre than these, and its colour inclines to a leaden gray.

It might be obtained in large crystals if the operation were conducted on a sufficiently extensive scale. Its exact crystalline form has not been determined; but in some modifications of the experiment, large hexagonal plates have been obtained, the edges of which were curved like those of diamond. It is harder than glass, but not so hard as topaz. Its specific gravity is 2·49; less, therefore, than that of its oxygen compound, for the specific gravity of quartz and of rock-crystal is from 2·6 to 2·8.

It is not at all affected by being heated in the alcohol and oxygen flame; but when fused with carbonate of soda, it decomposes the carbonic acid, carbon and carbonic oxide being produced, and the silicon itself being converted into silicic acid. It is slowly decomposed by caustic potash with evolution of hydrogen.

Geuther investigated the products of distillation of the Torbane Hill mineral. They are composed principally of an oil containing paraffine in solution. The oil consists of a carbohydrogen homologous with olefiant gas.

Gössmann and Atkinson communicate observations on lophine and some of its salts. For lophine they propose the formula $C^{42} H^{17} N^2$, which differs very little from that adopted by Fownes, one of the original discoverers of this base.

Breunlin proposes a new theory of the constitution of blue and green ultramarine. His view is, that ultramarine is a compound of a silicate of the formula of nepheline with a polysulphide of sodium. A translation of this paper was given in the preceding Number of this Journal.

On the coast of Oldenburgh large quantities of a kind of prawn are caught, which are made into an artificial manure by being dried and powdered. Hanstein has analysed this substance, and finds that it contains more nitrogen, but less phosphates than Peruvian guano; the deficiency in this latter constituent might be made good by the addition of bone-meal.

By the action of potash, cinnamic acid is decomposed into benzoic and acetic acids according to the following equation:—

$$C^9 H^8 O^2 + 2(KHO) = C^2 H^3 KO^2 + C^7 H^5 O^2 K + H^2.$$
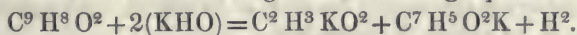
Chiozza, who observed this decomposition, found that when a mixture of aldehyde and oil of bitter almonds is saturated with

hydrochloric acid and gently heated, the hydride of cinnamyle or oil of cinnamon is produced. The author supposed that a change takes place here analogous to that observed when hydrochloric acid is passed into a solution of an organic acid in alcohol. Hydrochloric acid acting on one of the aldehydes may produce the compound $Cl\,C^2\,H^3$ or $Cl\,C^7\,H^5$; and this acting on the other aldehyde, would produce hydrochloric acid and hydride of cinnamyle. Adopting the notation used by Chiozza, the change would be thus expressed :—

$$Cl\,H \ + \ C^7\,H^6\,O = Cl\,C^7\,H^5 + H^2\,O,$$
Hydrochloric    Benzoic              Water.
acid.        aldehyde.

$$Cl\,C^7\,H^5 + C^2\,H^4\,O \ = \ Cl\,H \ + \ C^9\,H^8\,O\,;$$
Aldehyde.  Hydrochloric  Cinnamic
acid.      aldehyde.

or

$$Cl\,H \ + \ C^2\,H^4\,O = Cl\,C^2\,H^3 + H^2\,O,$$
Hydrochloric   Aldehyde.                Water.
acid.

$$Cl\,C^2\,H^3 + C^7\,H^5\,O \ = \ Cl\,H \ + \ C^9\,H^8\,O.$$
Benzoic  Hydrochloric  Cinnamic
aldehyde.    acid.      aldehyde.
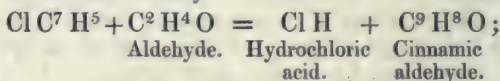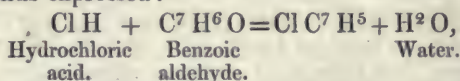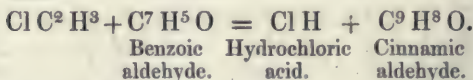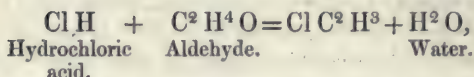
On passing sulphuretted hydrogen over dry formiate of lead, Wöhler observed the formation of a body crystallizing in fine needles. This substance has been investigated by Limpricht and Ritter, who consider it to be a new sulphur acid, and they propose for it the name *Thioformic acid.* In its formation, a portion of the formic acid liberated by the sulphuretted hydrogen from the formiate of lead decomposes with the excess of sulphuretted hydrogen into the new acid and into water :—

$$C^2\,H^2\,O^4 + 2HS = C^2\,H^2\,O^2\,S^2 + 2HO.$$
Formic acid.       New acid.
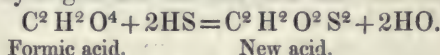
But the analyses made do not accord well with each other; and from the reactions made on the small quantity produced, no useful conclusions can be drawn. The subject requires more complete investigation.

In acting with nitric acid on oil of anise, it is generally assumed that hydruret of anisyle and oxalic acid are alone formed, thus :—

$$C^{20}\,H^{12}\,O^2 + 12O \ = \ C^{16}\,H^8\,O^2 \ + \ C^4\,H^2\,O^8 + 2HO.$$
Oil of anise.        Hydruret of anisyle.   Oxalic acid.
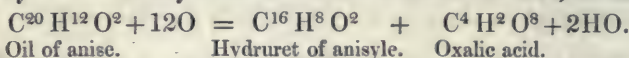
But Limpricht and Ritter show that there is a well-marked intermediary stage in the reaction at which an acid containing 20 equivs. of carbon is obtained :—

$$C^{20}\,H^{12}\,O^2 + 6HO + 4O = C^{20}\,H^{18}\,O^{12}.$$
Oil of anise.                    New acid.
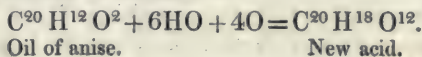
The acid has been named *Anisoïnic acid.* It crystallizes from an aqueous solution in small laminæ, but on account of its great solubility it is difficult to obtain in good crystals. Analyses of its soda, silver, and baryta salts have been made.

Limpricht proposes a new method for the formation of aldehydes. He considers that in the formation of acetone, 2 equivs. of acetate of lime are necessary; and he expresses the change as follows, using Gerhardt's notation :—

$$\left.\begin{array}{c} C^2(C^2H^3)O^2 \\ Ca \end{array}\right\}O^2 + \left.\begin{array}{c} C^2(C^2H^3)O^2 \\ Ca \end{array}\right\}O^2 = C^2Ca^2O^6 + \left.\begin{array}{c} C^2(C^2H^3)O^2 \\ C^2H^3 \end{array}\right\}$$

Acetate of lime.　　Acetate of lime.　Carbonate of lime.　Acetone.
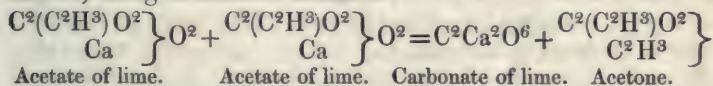
From one equivalent of the acetate, the radical acetyle, and from the other equivalent, methyle, unite to form acetone, the other elements forming carbonate of lime.

By heating together acetate of lime and formiate of lime, an acetone is obtained which contains an equivalent of hydrogen in the place of methyle; in other words, an aldehyde,—

$$\left.\begin{array}{c} C^2(C^2H^3)O^2 \\ Ca \end{array}\right\}O^2 + \left.\begin{array}{c} C^2(H)O^2 \\ Ca \end{array}\right\}O^2 = C^2Ca^2O^6 + \left.\begin{array}{c} C^2(C^2H^3)O^2 \\ H \end{array}\right\}$$

Acetate of lime.　　Formiate of lime.　　　　Aldehyde.
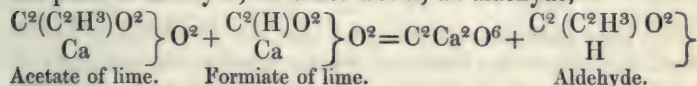
By distilling valerianate of lime with formiate, valeraldehyde is produced.

This method has only theoretical interest, for the aldehydes are produced only in small quantities, and so mixed with empyreumatic products that it is difficult to purify them.

Mohr proposes a modification of the method of estimating chlorides by a standard solution of nitrate of silver, by which the point where the whole of the chlorine is converted into chloride of silver is rendered more distinct.

Mohr adds to the solution of the chloride to be estimated a little chromate of potash solution, and then adds the nitrate of silver solution. As long as any chloride remains not precipitated, no chromate of silver is formed; but when all the chloride is precipitated, the addition of even a drop in excess of the standard solution causes the formation of some chromate of silver, which is instantly seen by its characteristic red colour.

In the April Number of the same Journal, Gorup Besanez gives the result of an investigation on the constituents of the contents of certain glands. Two years ago he announced the discovery, in the thymoid gland, of a weak basic body which he named thymine. This he has since investigated more closely, and finds that it is leucine. This body is an almost invariable constituent of the glandular juices, and is contained in largest quantity in the pancreas. He found in the pancreas a tolerably

large quantity of a new body, having the greatest similarity with leucine, both in appearance and chemical relation. It is homologous, and not identical with leucine. Its formula is $C^{10} H^{11} NO^4$; that of leucine is $C^{12} H^{13} NO^4$. The other conclusions he has arrived at have merely a special physiological interest.

Carius describes a compound of sesquioxide of manganese and sulphuric acid. It is obtained by heating binoxide of manganese with concentrated sulphuric acid. The excess of sulphuric acid is removed by washing with strong nitric acid, as the substance is decomposed by water, and the nitric acid is afterwards expelled by a gentle heat. It has the formula $Mn^2 O^3, 3SO^3$. It forms a deep green powder, without a trace of crystallization ; attracts moisture with great avidity, and must be kept in closed tubes. By treatment with water it is converted into hydrated oxide of manganese, and is indeed the best source for this body.

The Para nuts, the produce of a tree, the *Bertholletia excelsa*, which belongs to the family of the *Lecythodeæ*, contain upwards of 50 per cent. of a fatty oil. This oil, according to an investigation of Caldwell, consists of stearine, palmitine, and oleine.

According to Grimm and Ramdohr, the gas produced by the action of sulphuric acid on ferrocyanide of potassium is not pure carbonic oxide, but contains 11 per cent. of sulphurous acid. This appears to be reduced from the sulphuric acid by some of the carbon in the ferrocyanide.

Liebig gives a new method for silvering glass in the cold, which answers its purpose extremely well. The silvering liquid consists of an ammoniacal solution of nitrate of silver, containing caustic soda or potash. Before using, this is mixed with one-eighth or one-tenth of its quantity of a solution of sugar of milk, containing one part of sugar of milk to ten parts of water ; the glass plates to be silvered are then dipped into this mixture, and allowed to remain in about a quarter of an hour ; the silvering is then complete. The process is not costly ; a square metre of surface requires silver of the value of 5*d.* Liebig goes into the details necessary for ensuring success.

Glass can only be permanently gilded with the aid of heat. The gilding liquid is composed of a solution of double chloride of gold and sodium, containing some caustic soda. At the time of using, alcohol and æther are added to it, and a gentle heat applied. As in the former case, certain precautions are necessary, for which we must refer to the original paper.

Fleitmann communicates a process for estimating copper. The solution of copper, when freed from nitric acid and metals which might hinder the action, is precipitated by means of pure metallic zinc, and the precipitated copper well washed out. It is then dissolved in an acid solution of perchloride of iron: this takes place very rapidly, and gives twice the quantity of protochloride of iron, which may be estimated by permanganate of potash in the usual way.

Wicke investigated the quantity of butter contained in goat's milk taken in the morning, at noon, and in the evening. The quantity of butter at those times is respectively as $4·6 : 4·9 : 5·2$. The average quantity of butter is $4·9$ per cent. Cow's milk contains, according to Playfair, the same amount.

Pebal continues his investigations on the constitution of citric acid. It is not possible with our limited space to give any adequate idea of his paper, as he enters very much into theoretical considerations.

## VIII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from vol. xi. p. 255.]

Dec. 6, 1855.—Sir Benjamin Brodie, Bart., V.P., in the Chair.

THE following communication was read :—

"Results of the Examination of certain Vegetable Products from India."—Part I. ~ By John Stenhouse, LL.D., F.R.S.

Through the kindness of my esteemed friend Dr. Royle, I have been permitted to select such vegetable products from the extensive collection at the India House as seemed most likely to repay the trouble of investigation. My attention, during the last twelve months, has been chiefly directed to three of these vegetable substances; and the results of their examination I now take the liberty of submitting to the Royal Society, to be followed by those of the others as they may be completed.

#### *Datisca cannabina.*

The first of these substances which I examined consisted of a quantity of the roots of the *Datisca cannabina*, from Lahore, where this plant is employed to dye silk of a fast yellow colour. The roots, which had been cut into pieces about 6 or 8 inches long, were from a half to three-quarters of an inch in thickness. They had a deep yellow colour. A decoction of the leaves of the *Datisca cannabina* was examined by Braconnot in 1816, who discovered in it a crystallizable principle, to which he gave the name of *datiscine*.

Braconnot, of course, did not subject this substance to analysis, but he described its appearance and properties in an exceedingly accurate manner [*]. The observations of Braconnot had fallen into such entire oblivion, however, that for many years past, we find in most of the larger systems of chemistry the term datiscine used as synonymous with inuline. Thus in Brande's 'Chemistry,' vol. ii. p. 1168, we find it stated that a variety of names had been given to inuline, such as " dahline, datiscine," &c.

The bruised roots were extracted in a Mohr's apparatus by long-continued digestion with wood-spirit. The liquor obtained, which had a dark brown colour, was concentrated by distilling off a portion of the wood-spirit. The brown syrupy liquid remaining in the retort, on being poured into open vessels and standing for some time, deposited a resinous matter containing merely traces of a crystalline substance. When this syrupy liquid, however, was treated with about half its bulk of hot water, the greater portion of the brown resin was rapidly deposited, and the mother-liquor having been poured off and left to spontaneous evaporation, deposited a considerable quantity of an imperfectly crystallizable substance resembling grape-sugar. These crystals are datiscine containing a considerable amount of resinous matter. The datiscine, however, is rendered perfectly pure by treatment with a solution of gelatine, to remove any trace of tannic acid, and repeated crystallizations out of weak spirits of wine.

*Properties of Datiscine.*—Datiscine, when pure, is perfectly colourless. It is very soluble in alcohol, even in the cold, boiling alcohol dissolving any amount of it. By slow spontaneous evaporation, its alcoholic solutions yield small silky needles arranged in groups. Cold water does not dissolve much of it, but it is tolerably soluble in boiling water, the hot solutions on cooling depositing it in shining scales.

Datiscine is not very soluble in ether; but an ethereal solution, when evaporated, yielded larger crystals than were obtained by any other method. On adding water to an alcoholic solution of datiscine, no precipitate is immediately obtained, unless the solution is greatly concentrated; but on standing, very pure, pale yellow-coloured crystals of datiscine separate.

When datiscine is heated to about 180° C., it melts, and if the heat be increased, it burns, evolving an odour of caramel, and leaves a voluminous charcoal. If datiscine be heated in a close vessel while a stream of dry air is slowly passed over it, a small quantity of a crystalline substance sublimes. Datiscine and its solutions have a very bitter taste; and though it does not produce any change on test-paper, I think there is reason to regard it as a feebly acid body.

It dissolves in solutions of the fixed alkalies and ammonia, also in lime- and baryta-water. The addition of an acid to these solutions causes the precipitation of the datiscine.

The aqueous solution of datiscine is precipitated by neutral and basic acetates of lead, and chloride of tin. These precipitates have

* Annales de Chimie et de Physique, 1816, iii. 277.

a bright yellow colour. Salts of copper produce greenish, and those of peroxide of iron brownish-green precipitates. In consequence of the lead salts forming such gelatinous precipitates, they could not be employed for determining the equivalent of datiscine.

*Action of dilute Sulphuric Acid on Datiscine.*—When an aqueous solution of datiscine is boiled for a few minutes with very dilute sulphuric acid, it deposits a crystalline substance. On examining the solution filtered from the crystals, very distinct evidences of the presence of sugar were obtained. These experiments show therefore that datiscine, like salicine and similar bodies, belongs to the class of glucosides, and is a copulated compound of sugar and another substance which I shall call " datiscetine."

*Datiscetine.*—Datiscetine in its general appearance and properties closely resembles datiscine, but on a closer examination these two substances are found to differ essentially both in composition and properties. Datiscetine when pure assumes the form of fine needles which are nearly colourless. It is easily soluble in alcohol, a hot alcoholic solution, on cooling, depositing the greater portion in crystalline groups. It is almost insoluble in water, consequently datiscetine is precipitated from its alcoholic solutions by the addition of water. It dissolves in ether in almost any quantity, and is deposited on the evaporation of that liquid in needles. These properties of datiscetine enable us to obtain it in a tolerably pure state, even when very impure datiscine is employed in its preparation.

*Properties of Datiscetine.*—Datiscetine has no taste. When heated it melts like datiscine, but the heat required is much higher than for that body. It recrystallizes on cooling. By operating very cautiously, a portion of the datiscetine may be sublimed. The sublimate, however, appears to be altered datiscetine. Datiscetine when burned does not emit the odour of caramel. Datiscetine, like datiscine, dissolves in alkaline solutions, and is reprecipitated by the addition of an acid. An alcoholic solution of acetate of lead added to an alcoholic solution of datiscetine produces a deep yellow precipitate, which can be easily washed both by alcohol and water. This precipitate therefore was subjected to analysis, and from the results obtained, the formula $C_{30} H_8 O_{10} + 2PbO$ was calculated, which agrees with the formula ($C_{30} H_{10} O_{12}$) derived from the analysis of datiscetine.

*Analysis of Datiscine.*—It is difficult to calculate a formula for datiscine, the numbers of which shall agree with those found by analysis. When, however, the decomposition of datiscine into datiscetine and sugar is taken into consideration, it seems probable that the formula for datiscine is

$$\text{Datiscetine} + \text{sugar} = \text{datiscine}$$
$$C_{30} H_{10} O_{12} + C_{12} H_{12} O_{12} = C_{42} H_{22} O_{24}.$$
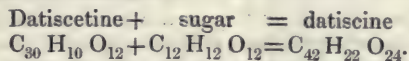
If the formula $C_{42} H_{22} O_{24}$ be correct, the decomposition of datiscine by dilute sulphuric acid would be analogous to that of salicine when treated in the same way.

Dilute hydrochloric acid, like dilute sulphuric acid, decomposes

datiscine, converting it into datiscetine and sugar. On boiling an aqueous solution of pure datiscine for some hours, traces of sugar could be detected, thus showing that a small portion of the datiscine had been decomposed.

It has been already shown that datiscine dissolves in cold solutions of potash without decomposition. When boiled, however, with a strong solution of potash for some time, decomposition takes place, and the precipitate, thrown down by the addition of an acid, has all the properties of datiscetine. In this respect, therefore, datiscine agrees with tannin and similar glucosides, which yield the same products when acted upon by acids and alkalies. Yeast and emulsine appeared to exert no action on solutions of datiscine.

*Action of Nitric Acid on Datiscine and Datiscetine.*—Cold nitric acid of the ordinary strength acts violently upon datiscetine, brown vapours are disengaged, and a resinous substance is produced, which is ultimately dissolved, forming a dark red liquid, which, when evaporated, yields crystals of nitropicric acid.

Datiscine treated in the same way yields nitropicric and oxalic acids.

When datiscine is boiled with dilute nitric acid it dissolves, and the solution obtained, when cooled, deposits pale yellow crystals, which agree in every way with the properties ascribed to nitrosalicylic acid.

On allowing datiscine to stand in contact with dilute nitric acid in the cold it gradually dissolves, the solution, when left to evaporate *in vacuo,* depositing a mixture of oxalic and nitropicric acids.

*Action of Potash on Datiscine and Datiscetine.*—It was stated in a previous part of this paper that datiscine and datiscetine dissolve in cold solutions of the alkalies without decomposition, and that datiscine, when boiled with potash, is decomposed with the formation of datiscetine. It only remained, therefore, to try the action of fused hydrate of potash. Datiscetine, when added in small successive portions to fused hydrate of potash, assumed a deep orange colour, and then dissolved with the evolution of hydrogen gas. When the disengagement of hydrogen had ceased, the mass was dissolved in water and supersaturated with hydrochloric acid. A partly resinous substance separated, which, by sublimation, yielded perfectly colourless, long crystals closely resembling benzoic acid. Their solution in water on the addition of perchloride of iron gave that deep violet tint which disappears on the addition of hydrochloric acid, and is so characteristic of salicylic acid.

*Action of Chromic Acid on Datiscetine.*—On distilling datiscetine with bichromate of potash and sulphuric acid a liquid came over, containing no oily drops, but having the smell of salicylous acid, and which, when tested with a persalt of iron, formed a purple-coloured solution characteristic of that acid.

It follows therefore, I think, from the experiments already detailed, that datiscine, like salicine, phloridzine, &c., is a glucoside, and that it approaches nearer to salicine than any other glucoside, with the exception of populine, yet known.

I will conclude this account of datiscine by proposing the following practical application. As is well known, the colouring matter of madder, when boiled with dilute sulphuric acid, is changed into sugar and garancine, a new dye-stuff, which, for many purposes, is found superior to that originally present in the madder. Within the last twelve months Mr. Lieshing, by treating the colouring matters in weld and quercitron bark with dilute sulphuric acid, has resolved them into new colouring matters, which are but slightly soluble in water, and are found nearly three times more powerful as dye-stuffs than the original colouring matters from which they had been produced.

As datiscine, when boiled with dilute sulphuric acid, undergoes a perfectly similar transformation, being resolved into sugar and datiscetine, which has a much higher colouring power than the datiscine which has produced it, I have not the least doubt that silk dyers, who may hereafter employ solutions of *Datisca cannabina*, will find it highly advantageous to convert their datiscine into datiscetine by boiling it with dilute sulphuric acid.

## *Oil of the* Ptychotis Ajowan.

The *Ptychotis Ajowan* is an umbelliferous plant well known in India for its aromatic and carminative properties. When its seeds are distilled with water they readily yield between five and six per cent. of an essential oil resembling in smell that of oil of thyme. When this oil is left in shallow vessels to spontaneous evaporation at low temperatures, it deposits a large quantity of beautiful crystals, which are identical with the stearopten brought from India by the late Dr. Stocks, and described by me in a short notice in the December Number of the ' Pharmaceutical Journal' for 1854.

The crude oil was rectified, and the portion which came over between $160°$ C. and $164°$ C. was collected separately and carefully rectified over sodium. Its boiling-point was found to be $172°$ C., and its composition isomeric with oil of turpentine, namely

$$C_{10} H_8 \text{ or } C_{20} H_{16}.$$
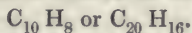
The stearopten obtained from the less volatile portion of the oil was purified, and formed large flat rhombohedral crystals, which have been carefully measured by Professor Miller of Cambridge. When subjected to analysis it gave the formula

$$C_{20} H_{14} O_2 \text{ or } C_{20} H_{13} O_1 HO.$$
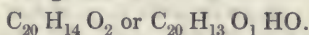
In the notice of the stearopten of this oil which appeared in the 'Pharmaceutical Journal,' from the examination of the small quantity then at my disposal, a different formula was deduced, which I now withdraw, and substitute the preceding formula, $C_{20} H_{14} O_2$, in its stead.

When the crystalline stearopten is digested for eight or nine days with the strongest nitric acid, it is gradually converted into a crystallizable acid, apparently containing no nitrogen. This acid is but slightly soluble even in boiling water, but is deposited in needles on the cooling of the solution. It is very soluble in alcohol

and ether, from which it is deposited in crystals. Both its silver and its baryta salts are crystallizable, and appear very stable. It seems to me to be a new acid, and this I hope soon to be able to ascertain.

When the stearopten is gently warmed with oil of vitriol it dissolves, and, on cooling, solidifies into a crystalline mass. This new compound, which is a copulated acid, dissolves readily in hot water, and, on cooling, is deposited in large scaly crystals, of a mother-of-pearl lustre. It forms both a crystallizable lead and baryta salt.

When the stearopten is distilled with a mixture of peroxide of manganese and sulphuric acid, it yields a substance exceedingly analogous in almost every respect to the thymöil obtained by Lallemande by subjecting the stearopten of oil of thyme to similar treatment. As the details of Lallemande's experiments have not yet been published, it would be premature to pronounce with absolute confidence on the identity of the stearoptens from the *Ptychotis* and oil of thyme; but if not identical, as I rather apprehend they are, they are certainly extremely similar bodies*.

### *Gum of the* Gardenia lucida, *Roxb. (the Decamalee Gum of Scinde).*

The specimen of this gum on which I operated was evidently very old. It formed a hard, dry mass, of a dark brown colour, with numerous patches of a greenish-yellow matter disseminated through it. It had but a faint odour, unless freshly fractured or gently heated, when it smelt like the urine of the cat.

A comparatively recent specimen of this gum, which I saw in the hands of the late Dr. Stocks, had merely the consistence of candied honey, and an exceedingly offensive odour. Dr. Stocks informed me that the fresh gum was employed as a dressing for wounds, as it kept off the flies. The resin was digested in strong spirit of wine, till a saturated solution was obtained. This, on cooling, immediately deposited some yellow amorphous flocks. These were separated by filtration, and the clear liquid slowly evaporated *in vacuo*. On standing a few days, it deposited a quantity of golden yellow slender crystals, about half an inch in length. The crystals had considerable lustre, and were very brittle. To this crystalline substance I purpose giving the provisional name of *gardenine*. Gardenine is nearly insoluble both in cold and hot water. It dissolves pretty readily in alcohol, but much less readily in ether; ether yielding bright yellow solutions, out of which it crystallizes on cooling. Alkalies, such as ammonia, do not appear to increase

* Since this paper was communicated to the Royal Society, a notice of the Ptychotis oil, by Dr. Haines, of the Bombay College, was read before the last meeting of the Chemical Society. Dr. Haines has generally arrived at similar conclusions to my own. He regards, however, the carbohydrogen portion of the oil not as isomeric with oil of turpentine, but as $C_{20} H_{14}$.

His formula for the stearopten is the same as that given in this paper; and he regards it as identical with Lallemande's thymole.

Dr. Haines, however, appears not to have observed the crystalline acid produced by the action of nitric acid on the stearopten.

its solubility. It is more soluble in hot hydrochloric and sulphuric acids than in water, and is precipitated, apparently unchanged, on the addition of water. Its alcoholic solutions give no precipitate with ammonio-nitrate of silver, or with basic acetate of lead. When gardenine is digested with concentrated nitric acid, it is rapidly decomposed; nitropicric acid, but apparently no oxalic acid, being produced.

Unfortunately, from the very small quantity of resin at my disposal, I was unable to prepare a sufficient amount of the gardenine either to subject it to analysis or to examine it more particularly. Dr. Royle has, however, commissioned a large quantity of the resin from India, which I trust will ere long enable me to complete its examination. Gardenine appears to belong to the tolerably numerous class of indifferent crystallizable resins, of which it is certainly one of the most beautiful.

January 10, 1856.—Admiral Beechey, V.P., in the Chair.

The following communication was read:—

"On the Existence of Multiple Proportion in the quantities of heat, or equivalent alteration of internal space of bodies, caused by definite changes of state as produced by Chemical Combination or otherwise." By Thomas Woods, M.D.

Gay-Lussac having shown that the combining proportions of gases and vapours are either equal in volume or some multiple of each other, and other chemists, particularly Playfair and Joule, having extended the same law to solids and liquids, it is evident that *specific volume* of its combining equivalent is characteristic of matter. But as every substance is composed of matter and space, or of particles with some distance between them, as is shown by expansion and contraction, whenever volume is altered there must be either an addition to or subtraction from the internal space of the body. This alteration of volume is evident in the case of bodies expanding or contracting by gain or loss of heat; but in chemical combination, where alteration of internal space must take place also (as shown by change of temperature, and because *specific* volume being characteristic of matter, this volume must change when the matter changes, by a substitution of a mixture of two kinds of matter for one), in chemical combination, I say, this alteration of internal space is not so plainly demonstrable. Still, in the change of temperature, we have not only an evidence, but a measure of the change of state of combining bodies. For, the phenomena of heat being produced by a *dual* force acting equally in opposite directions, one body cooling or contracting as another becomes heated or expands, it may be taken for granted that whenever heat or expansion is found in one body, the opposite change is occurring in some other. Now, regarding this proposition as true, it is intended to be proved from the combinations of oxygen that the internal space of a substance is lost and gained in multiple proportion, in the definite changes of state of bodies, such as in the condensation of vapours into liquids,

and liquids into solids, and the reverse; and also in chemical combinations and decompositions; and therefore that the *space* as well as the *matter* of which volume is composed can be only added to or taken away in what is called its combining equivalent.

In order to find whether the heat of chemical combination, which is taken as equivalent to the alteration of internal space, is equally produced by the same substance uniting with others, or if not, if it is given out in multiple proportion, oxygen is made to combine with several other simple bodies, and the alteration of temperature noted.

The method of oxidizing these substances, the details of each process being given in the paper, consisted in dissolving them in some suitable menstruum,—for instance, in sulphuric acid, liquor of potass, and nitric acid. When the two former are used, water is decomposed to oxidize the dissolved body; in the last case, the nitric acid is resolved into oxygen and binoxide of nitrogen, the former of which unites with the substance to be oxidized, the latter escaping. Other combinations and decompositions at the same time take place (as detailed in the paper), and being taken into account (decompositions absorbing as much heat as is produced by the combination of the constituents), the alteration of temperature by the oxidation alone is arrived at.

In this manner eighteen different metals were oxidized, but the heat of oxidation was obtained satisfactorily only with twelve. Other experimenters (Favre and Silbermann and Andrews) have, with a different object in view, found the heat of oxidation of fourteen other substances; their conclusions are added as being from unprejudiced sources, and the result of all the experiments is brought together in a table, in order to see whether the law of multiple proportion exists. The numbers found by the different experimenters are all calculated to the same standard. The unit of heat is the amount necessary to raise the temperature of 1000 grains of water $1°$ Fahr., and the quantity of the metal oxidized is an equivalent of each, oxygen $= 1$.

To find whether the law extends to change of state when no chemical combination takes place, the amount of heat given out by the condensation of an equivalent of steam, and by the solidification of an equivalent of water, is given. The following is the table giving the thermal equivalents of the several substances, the names of the experimenters, and the ratio of proportion. It is to be remarked, that the condensation of steam being in multiple proportion with the other thermal equivalents, the expansion of all other bodies into vapour must be included; for I showed in the Philosophical Magazine for January 1852 (page 48), that all bodies expand into vapour in some multiple of their atomic volumes, and their atomic volumes being in ratio also, their expansion or gain of internal space in this definite change must be in multiple proportion.

TABLE showing the quantity of heat produced in 1000 grs. of water by the oxidation of an equivalent of each substance, $O=1$.

| Name of substance oxidized. | Units of heat. | Ratio. | Name of Experimenter. |
|---|---|---|---|
| Latent heat of ice ... | ·1603 | ·1603 | |
| Latent heat of steam | 1·287 | 8 times ·1603 | |
| Iodine.................... | ·8 | 5 times ·1603 | Woods. |
| Chlorine............... | −1·6 | .................. | Favre and Silbermann. |
| Nitrogen............... | 1·6 | twice ·8 | Woods. |
| Silver .................. | 1·6 | .................. | Woods. |
| Selenium ............ | 2·7 | .................. | Favre and Silbermann. |
| Mercury .............. | 2·4 | 3 times ·8 | Woods. |
| Palladium ............ | 2·42 | .................. | Woods. |
| Molybdenum ......... | 3·38 | | Woods. |
| Carbon ............... | 3·3 | 4 times ·8 | Favre and Silbermann. |
| Arsenic .............. | 4·8 | .................. | Favre and Silbermann. |
| Antimony ............ | 4·8 | .................. | Woods. |
| Copper ............... | 4·9 | 6 times ·8 | Favre and Silbermann. |
| Cobalt ............... | 4·8 | .................. | Woods. |
| Bismuth .............. | 4·82 | .................. | Woods. |
| Nickel.................. | 6·5 | | Woods. |
| Lead .................. | 6·2 | 8 times ·8 | Favre and Silbermann. |
| Hydrogen ............ | 7·8 | .................. | Favre and Silbermann. |
| Tin ..................... | 8·0 | .................. | Mean of Andrews and Favre and Silbermann. |
| Phosphorus ......... | 8·1 | 10 times ·8 | Favre and Silbermann. |
| Cadmium ............ | 8·18 | .................. | Woods. |
| Iron ................. | 7·95 | .................. | Mean of Andrews and Favre and Silbermann. |
| Zinc ................... | 9·6 | 12 times ·8 | Favre and Silbermann. |
| Manganese............ | 10·4 | 13 times ·8 | Woods. |
| Barium ............... | 12·8 | 16 times ·8 | Woods. |
| Aluminium............ | 16·16 | 20 times ·8 | Woods. |
| Sodium ............... | 17·5 | 22 times ·8 | Favre and Silbermann. |
| Potassium ............ | 17·3 | | Favre and Silbermann. |

*Note.*—I proved in a paper published in the Philosophical Magazine for October 1851, that " the decomposition of a compound body absorbs as much heat as the combination of the elements originally produced." I believe I was the first to prove this as a general proposition, and, by so doing, laid the foundation of almost all the thermochemical researches since carried on ; for, as far as I am aware, no process which took decomposition into account was used before my paper was published.

In a paper read to the British Association at Belfast, and published in the Philosophical Magazine for November 1852, I proved that the intensity of chemical affinity might be measured by the quantity of heat produced by the combination.

As regards the first of these papers, Mr. Joule published in the Philosophical Magazine for June 1852, a memoir proving exactly the same proposition, but giving me the merit of priority in a preliminary remark. It is, however, singular that Favre and Silbermann bring forward in 1853 (*Annales de Chimie et de Physique,*

vol. xxxvii. p. 507) the very same experiments to prove the same fact, and give it as their own.

As regards the second paper. In six months after its publication, Messrs. Favre and Silbermann (*Annales de Chimie et de Physique*, vol. xxxvii. p. 484) prove the same truth with the same experiments, using exactly the same metals, and give their memoir as producing an original idea.

I notice these coincidences here as being remarkable, and because the propositions contained in the paper referred to are the ground-wrk of the present experiments, and also with a view to prevent an unconscious repetition on the part of Messrs. Favre and Silbermann.

---

### GEOLOGICAL SOCIETY.

May 28, 1856.—Colonel Portlock, Vice-President, in the Chair.

The following communications were read :—

1. " On the Silurian Rocks of Wigtonshire." By J. C. Moore, Esq., F.G.S.

The objects of this communication were,—1st, to point out a remarkable arrangement in the rocks which form the peninsula between the Mull of Galloway and Corswall Point ; and, 2ndly, to show the relative positions of the graptolitic schists of Wigtonshire and of the coarse conglomerate and limestones of Ayrshire. The author had already shown in a former communication that the rocks from the Corswall Lighthouse for a great distance to the south have, in the main, a southerly dip ; and that, after passing to the south of Port Patrick, the dip is found to be reversed, that is, to the north. By subsequent visits to this coast, Mr. Moore has been enabled to offer a detailed account of the numerous anticlinal and synclinal folds, exhibited more or less perfectly along this coast-section, a distance of more than 30 miles. These foldings of the beds have the anticlinals (the arches of which are occasionally preserved) thrown northward along the northern portion of the section ; their shorter or northern sides being vertical or nearly so, and their longer or southern sides being inclined at varying angles, often not more than 30°. The opposite conditions obtain in the southern portion of the section towards the Mull of Galloway. Sometimes the flexures are very numerous ; in one place as many as fifteen occur in five miles.

On the east side of the Bay of Luce the coast-section, parallel to the above, affords confirmations of the same structure. The strata are undulated, with the axes of the folds toward the south, and the long sides of the folds gently dipping northward. The east coast of Loch Ryan may be regarded as a northern prolongation of this section, and, in conformity with the Corswall Point section, exhibits a section of rocks folded over towards the north.

At Corswall Point, and in the corresponding section in Loch

Ryan, are coarse conglomerates of granite, porphyry, &c., and towards the middle of the section are schists with graptolites, which latter, from the general southern dip of the strata and the frequent absence of the axial arches, appear to overlie the former. The graptolite-schists, however, are the equivalent of the graptolitic and anthracitic schists of Dumfriesshire, Selkirkshire, and Peebleshire, and are really lower in stratigraphical position than the conglomerates of Corswall Point and the conglomerates and limestone of the south of Ayrshire, of which the Wrae limestone, with its associated conglomerate, is the counterpart. The abnormal relative position of the conglomerates and graptolite-schists on the western coast of Wigtonshire is due, in the author's opinion, to an inversion of the rock-masses, similar to the great inverted flexures of strata seen in the Ardennes, the Eifel, the Appalachians, &c. This great folding up of the Silurian strata has thrown them into axial folds which have an E.N.E. strike, similar to that prevailing over the north of Ireland, the south of Scotland, and even in Norway, and must be due to some physical cause which has operated on a magnificent scale. In the author's opinion, the granites of Cairnsmuir, Criffel, &c. have been protruded subsequently to the great movements above alluded to, and have somewhat deranged the E.N.E. strike.

2. " On the action of Ocean-currents in the formation of the Strata of the Earth." By C. Babbage, Esq., F.R.S. Communicated by W. H. Fitton, M.D., F.R.S., F.G.S.

In the first part of this paper the author traced out the laws which regulate the distribution of very finely divided earthy matter, borne outwards from river-mouths and sea-cliffs into the ocean-currents, over extensive areas. The time that a particle of matter requires to fall through a given distance in a resisting medium depends—

1st. On the specific gravity of the particle itself.

2nd. On its greater or less magnitude.

3rd. On its form.

4th. On the law of the resistance of the medium through which it falls.

These several points were treated of by the author, who then proceeded to show under what conditions certain finely triturated substances, of given size and composition, suspended in a current of a given velocity, would be deposited in a sea of a given depth. He noticed also how the uniformity of a stratum might be interfered with by the varying conditions both of the sediment and of the sea-bottom. Altered relations between the specific gravity, the shape, and the size of the particles, when duly adjusted, render ocean-currents capable of either separating mixed substances, or of combining together different substances. Hence endless combinations arise from the variation of these conditions.

Local elevations and depressions of the sea-bed, on which sediment brought from a distance is deposited, were pointed out as probable causes of irregularities in stratified deposits,—giving origin,

indeed, either to outliers or disconnected masses, which might be sometimes supposed to have been due to subsequent denudation.

Sedimentary matter carried by ocean-currents to the profound depths of the ocean subside into these depths beyond the reach both of currents and of wave-action. The downward motion becomes continually diminished, and the particles ultimately come to absolute rest, or move through water of increasing density with excessive slowness, so as to cover the ocean-bottom with an incoherent pulpy mass of fluid mud, of great thickness, and less dense for the most part in the upper than in the lower part,—or to form a similar mass of sediment suspended in mid-water.

It was also pointed out that in the immense period of time during which this sediment is subsiding into the profound ocean-depths and massing itself into a mud-bed, various hydrographical changes might take place and cause new currents to bring different sediments over the same area, which newer deposits would descend into and be mingled with the older precipitates.

The author proceeded to treat of the effects of an alteration of isothermal surfaces, caused by the interference of this more or less suspended mud-cloud with the conduction of heat from the earth's surface. Consolidation of the lower strata would be caused by the isothermal surfaces below the ocean rising upwards. Currents of heated water, similarly caused, might variously disturb the sediment and give it flexuous stratification. Heated water might be retained in portions of the sedimentary masses, and alter by its solvent power the constituent materials ; or the heated water might be converted into steam, or generate permanent gases, which might derange or alter the suspended material in various ways. If the sediment had not reached the bottom, but formed a freely suspended mud-cloud in mid-ocean, the effect of the interposed bed of fluid mud impeding the upward progress of heat from the lower region would necessarily be to increase the heat of the water below the mud, and thus place the sediment between the upward pressure of the heated water and the downward pressure of the overlying water. The ocean above would cease to derive its usual supply of heat from below, and become climatally altered. The now consolidated mud-bed would of its own weight either sink bodily down, and take different positions according to its consistency and the form of the ocean-bottom, or it would be contorted and broken through from the effect of the accumulated heat below. In tracing the results of this upward pressure and bursting, the author observed that on the enormously thick and partially consolidated stratified mass one or more weak points would admit of the formation of elevated domes, and that from the bursting of one of these domes, in a sea of much greater length than breadth, a vast wave would be propagated through the plastic matter, which would advance and be followed by others less perhaps in degree. As the original wave advanced, the diminishing depth of the ocean would cause the head of the wave to advance with greater speed than its base, impeded by friction on the ocean-floor, and give it its advancing form and a

steeper declivity in front than on its hind side; this might be carried so far that the foremost wave may even double itself over, and yet, owing to the plasticity of the mass, there might be no breach of continuity. To the transmission of such impulses through semi-consolidated strata, the author refers for an explanation of the overlapping and inversion of strata seen in the Appalachian and other mountain-ranges.

The paper concluded with remarks on the indications of the age, and causes influencing the structure of deposits, such as cleavage, &c., in connexion with the foregoing observations on sedimentary formations, and as illustrating, with them, some of the consequences of several physical causes which act through vast intervals of time upon the strata forming the crust of the earth.

June 4, 1856.—Colonel Portlock, Vice-President, in the Chair.

The following communications were read:—
1. "Notice of the Keuper Sandstone and its Fossils found at Leicester." By James Plant, Esq. Communicated by J. W. Salter, Esq., F.G.S.

The author met with the following section of the Keuper and overlying beds in the immediate vicinity of Leicester, in a N.W. direction:—1. alluvial deposits, containing remains of Deer and Oxen, with nuts, leaves, &c., 10 feet: 2. gravels and clays, with Elephants'-teeth, oolitic detritus, and boulders of syenitic and metamorphic rocks, 30 feet: 3. laminated clays and blue marls, 60 feet: 4. thin strata of Keuper shales, grey marl, and sandstone, with numerous irregular branched casts on the surfaces, usually called Fucoids, but referred by Mr. Plant to *Gorgoniæ*, 80 feet: 5. fine silicious white sandstone, in beds about 3 feet thick, occasionally intercalated with by a thin band of carbonaceous matter, 20 feet: 6. clays, thin sandstones, and grey marls, 35 feet: making a total of nearly 200 feet of Keuper beds. Besides the ramiform surface-markings above alluded to, the Keuper here exhibits ripple-marks, foot-tracks, and other superficial impressions, and yields some remains of plants, referred to *Echinostachys* and *Voltzia*, also numerous specimens of the little *Posidonomya minuta*, together with teeth and spines of Fish, fragments of bone, and coprolites. The author remarked that the Leicester Keuper most closely agrees with the same formation in Gloucestershire, as described by Murchison and Strickland, even in lithological characters. The Keuper beds are divisible into three chief members in each district,—the upper, thin sandy shales, with way-boards of green marl,—the middle, thick beds of soft white sandstone,—and the lower like the upper.

2. "Remarks on the Keuper of Warwickshire." By the Rev. P. B. Brodie, M.A., F.G.S.

The old quarries on Shrewley Common, described by Murchison and Strickland, are now closed, but excavations near the Canal at Shrewley and Rowington have lately afforded sections and some fossils of the Keuper beds, as follows:—1. Sandstone and marl, a

few inches ; 2. Reddish marl, 4 inches ; 3. Green marl, 4 inches ;
4. Light-coloured, fine-grained sandstone divided by marls, 3 feet,
with *Posidonomya minuta,* Fish-bones, and Ripple-mark ; 5. Gritty
sandstone, with teeth of *Acrodus,* and fish-bones and spines, 8 inches ;
6. Green marl, ½ inch ; 7. Hard sandstone, with *Posidonomya,* 10
inches ; 8. Sandstone divided by green marls, 10 feet, with remains
of Plants, *Voltzia* and *Calamites* ? 9. Red marl. Bones of *Labyrin-
thodon* were met with ; and Foot-tracks also occur, but their place
in the series was not determined. Fucoidal markings are frequent
throughout ; and ripple-mark is common in the sandstones. The
Warwickshire Keuper very nearly agrees with that of Worcestershire,
as described by the Rev. Mr. Symonds, but at Pendock the bed
No. 5 is coarser, is still richer in osseous remains, and contains car-
bonaceous matter.

3. " On a new genus of Cephalopoda, *Diploceras (Orthoceras
bisiphonatum* of Sowerby). By J. W. Salter, Esq., F.G.S.

In this communication the author pointed out the apparent rela-
tions of this peculiar form, which has been figured in the ' Silu-
rian System ' and in ' Siluria.' It possessed ordinary septa, pierced
by an excentric beaded siphuncle, and also had a deep lateral cavity
passing down side by side with the siphuncle, and affecting at least
seven, if not more of the uppermost septa.

Mr. Salter remarked that the structural peculiarities of *Ortho-
ceras paradoxicum* and of *Gonioceras* might elucidate the character of
the shell in question ; but he thought that the study of *Ascoceras* and
*Cameroceras* would be still more likely to throw light on the subject.
Mr. Salter also described a new species of *Ascoceras (A. Barrandii),*
found not long since in the upper Ludlow rock, at Ludlow, and at
Stansbatch in Herefordshire.

4. " On an *Orthoceras* from China." By S. P. Woodward, Esq.,
F.G.S.

The specimen in question was one of several that were imported
from China in 1854 by Mr. D. Hanbury. They are longitudinal
sections in thin, polished slabs of limestone, and were obtained by
Mr. Lockhart, of Shanghae, from some place 200 miles distant.
They seem to have been used as screens, having been mounted in
carved wooden frames with stands. Mr. Woodward described in
detail the appearances presented by the fossil in question, which is
probably of Devonian age, and offers some interesting facts as to
the original relations of the siphuncle, the lining membrane of the
shell, and other parts of the animal. The specimen exhibits appear-
ances attributed to the gradual separation and ultimate collapse of
the lining membrane from the walls of the air-chambers towards the
apex of the spire, during the life-time of the animal.

He stated that the changes which this specimen has undergone
appear to be these,—1. When buried in the sea-bed, the mud entered
the siphuncle and filled the interior of those chambers in which the
siphuncle was incomplete. 2. Water, containing carbonate of lime
in solution, penetrated the air-chambers and other closed spaces,

coating all the inner surfaces with tufa.   3. The shell was dissolved and removed before the consolidation of the surrounding mud, which thus obtained access to all those cavities whose calcareous lining was incomplete.   4. The cavities which the mud could not enter were filled, or nearly filled with crystalline carbonate of lime.   Mr. Woodward concluded his paper with remarks on the structure and filling up of the siphuncle in *Actinoceras* and other allied forms of *Cephalopoda*.

5. " On Trap-dykes intersecting Syenite in the Malvern Hills, Worcestershire." By the Rev. W. S. Symonds, F.G.S.

The author described the dykes of greenstone and trap traversing syenite in a quarry between the Winds Point and the Obelisk. The syenite is altered by contact with the dykes, and the appearances closely resemble those observed by the author in the syenite on which the great bonfire was made on the Beacon Hill in January last.

6. " On the Movement of Land in the South Sea Islands."   By James G. Sawkins, Esq., F.G.S.

The author when visiting the Friendly Islands in 1854 collected information relative to an earthquake which had lately occurred, accompanied by a sinking of the eastern and north-eastern portion of Tongataboo, and an uprise on the western coast, and the appearance of an island or shoal of black sand at a distance of thirty miles to the westward. Another earthquake occurred afterwards at Ninapoon, an island to the northward, which was not felt at Tongataboo. The author also referred to other indications of local movements of land in the Pacific Islands.

7. " On the possible origin of veins of Gold in Quartz and other rocks." By L. L. B. Ibbetson, Esq., F.R.S., F.G.S.

Having mixed a solution of gold in nitromuriatic acid with five times its weight of water, and placed it in a Berlin evaporating-dish on a thick sheet of copper over a gas-lamp, the author observed a crack in the basin, which was increasing. On transferring the solution to another basin, he found that the crack presented a vein of gold ; the pure gold forming small nodular masses along the fissure, both inside and out, and resembling veins of gold in auriferous quartz-rocks.   Under the circumstances of the low temperature at which the solution was being evaporated, the diluted state of the solution still left unevaporated, and the difference of the appearance of the nodular form of the gold-vein from the usual appearance of the metallic gold obtained by evaporation from such a solution, the author thought it worth while to describe and exhibit the specimen to the meeting.

## IX. *Intelligence and Miscellaneous Articles.*

### THE ABSORPTION OF HEAT BY DECOMPOSITION.

#### *To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,                                    Parsonstown, June 1856.

IN an abstract of a paper sent by me to the Royal Society, published in the 'Proceedings' for January last, I claim the discovery of the law that " *decomposition* absorbs as much heat as the *combination* of the elements originally produced." In the 'Proceedings' of the Society for March following, Mr. Joule disputes my claim, and refers to papers of his published in the Philosophical Magazine, October 1841. He also says he sent a memoir to the French Academy in 1846, which however was not published until 1852, in the Philosophical Magazine.

As to the paper published in the Philosophical Magazine for October 1841, after carefully reading it, I cannot find anything in it which proves the law I have mentioned. Mr. Joule proves in it that the heat produced in a conductor by a galvanic current is in proportion to the resistance and the square of the intensity of the current, meaning by "intensity" the quantity passed through a certain resistance in a given time. When he caused the current to pass through an electrolyte, he found that this law did not hold good, that the same amount of heat was not produced as when it passed through a solid body. He therefore concluded,—and he says he found by an experiment, which he does not describe—that part of the intensity of the current is used up in producing electrolysation, and therefore cannot be employed in evolving heat. Now, so far from any proof being here offered that " decomposition absorbs as much heat as combination produces," an exactly opposite doctrine is taught; for the conclusion to be arrived at is, that if the intensity of the current was *not* lessened by electrolysation, the same amount of heat would be produced in the electrolyte as in the solid conductor offering the same resistance, or in other words, that if the same quantity of galvanic current from the same source passed through an electrolyte and through a wire (an experiment easily made by accommodating the length of the wire to the resistance of the electrolyte), the same quantity of sensible heat would be generated. But this is quite opposed to the truth. In my experiments published in the Philosophical Magazine for October 1851, and in Mr. Joule's, published in the same Magazine for July 1852, it is shown that the current under these circumstances developes different amounts of heat, and that the difference is the quantity absorbed by decomposition.

With respect to the memoir sent by Mr. Joule to the French Academy in 1846, although it proves the law referred to exactly in the same manner in which my paper does, it cannot interfere with my claim to the discovery, as it was not published for nine months after mine. In all cases I believe the first publication of a discovery decides the priority. The publication of his paper nine months

after mine can only affect me by offering additional evidence of the truth of my experiments.

Since I published my paper in the Philosophical Magazine for October 1851, thermo-chemical researches have been extensively carried on, and all based on the truth of the proposition I there proved, viz. "that decomposition of a compound body absorbs as much heat as combination of the elements produced." Before that date no such experiments were thought of, that is, the amount of heat produced by the union of elements was in no instance calculated from any process taking decomposition into account. The reason why the combustion of a compound body, such as alcohol, does not produce as much heat as the elements would when separately ignited, was not even understood,—proofs that philosophers were not aware of the principle before my paper was published, and consequently had not perceived it in Mr. Joule's.

I published a paper in the Philosophical Magazine for November 1852, proving that the intensity of the affinity of elements for oxygen might be measured by the quantity of heat their combination with that body produces. On referring to Mr. Joule's paper published in vol. xx. of the Philosophical Magazine for 1842, I find that he has there proved the same proposition. If I had known of this paper when I published mine, I would have quoted it in preference ; but at the same time I must say, that if the proposition be true and of importance, I have a right to claim its establishment ; for although Mr. Joule in 1842 brings it forward, in 1843 he rejects it. (See Phil. Mag. vol. xxiii. page 442.) It was therefore the more necessary subsequently to show it was correct.

<div align="center">

I have the honour to be,

Your obedient Servant,

Thomas Woods.

</div>

## ON HEAT AS THE EQUIVALENT OF WORK.

Hoppe has contributed a memoir upon this most interesting and important subject, which places the analytical theory in a remarkably clear, simple, and general point of view, so far at least as it relates to permanent gases. We shall give the author's investigation *in extenso*, making however a slight change in the symbols employed, so as to assimilate them to those usually employed by writers on the calculus in French or English. The temperature $\tau$ of an enclosed permanent gas may be expressed as a function of the pressure $p$, and the volume $v$ by the following formula, which is a combination of the laws of Gay-Lussac and Mariotte :—

$$pv = \frac{ma}{\delta}(1 + a\tau). \quad\quad\quad\quad (1)$$

In this formula $m$ represents the mass of the gas, $a$ any definite pressure, that of one atmosphere for instance, $\delta$ the density at the temperature $0°$ and pressure $a$. If we consider the temperature to

be measured by the increments of volume of the gas itself, $\alpha$ will be absolutely constant, and Mariotte's law will be the only fact resting upon experience.

Let $\vartheta$ denote the quantity of heat which the mass $m$ of the gas must receive from without, in order to produce any changes whatever in $p$, $v$ and $\tau$. Then considering $p$ and $v$ as independent variables, and $\tau$ as a function of both, we can make

$$\frac{d\vartheta}{dv} = mc\frac{d\tau}{dv}; \quad \frac{d\vartheta}{dp} = mc'\frac{d\tau}{dp}.$$

The magnitudes $c$ and $c'$, defined in this manner, express the capacities for heat at a constant volume and at a constant pressure, and may for the present be regarded as constant. Substituting in these expressions the values of the partial differential coefficients of $\tau$ as obtained from (1), namely,

$$\frac{d\tau}{dv} = \frac{\delta p}{ma\,\alpha}; \quad \frac{d\tau}{dp} = \frac{\delta p}{ma\,\alpha},$$

we have

$$\frac{d\vartheta}{dv} = \frac{\delta cp}{a\,\alpha}; \quad \frac{d\vartheta}{dp} = \frac{\delta c'v}{a\,\alpha}.$$

and hence for the total differential of $\vartheta$

$$d\vartheta = \frac{\delta}{a\,\alpha}\,(cp\,dv + c'v\,dp).$$

If now the gas pass from one state to another, so that $p$ and $v$ change according to any definite law, $p$, $v$, and $\vartheta$ become functions of each other, and we have

$$\vartheta = \frac{\delta}{a\,\alpha}\left(c\int p\,dv + c'\int v\,dp\right). \quad . \quad . \quad . \quad . \quad (2)$$

If $\tau_0$ denote the initial temperature, then from the equation (1) we have

$$d\tau = \frac{\delta}{ma\,\alpha}\,(p\,dv + v\,dp);$$

and if we integrate between the same limits as those to which $\vartheta$ is referred, and multiply by $mc'$, we have

$$mc'(\tau - \tau_0) = \frac{\delta}{a\,\alpha}\,c'\left(\int p\,dv + \int v\,dp\right), \quad . \quad . \quad . \quad (3)$$

which, subtracted from equation (2), gives

$$\vartheta - mc'(\tau - \tau_0) = \frac{\delta}{a\,\alpha}\,(c - c')q, \quad . \quad . \quad . \quad . \quad (4)$$

where $q = \int p\,dv$ expresses the work done in the change of state.

The result is therefore as follows :—" The quantity of heat communicated to a gas during any change of volume and pressure consists of two parts, one of which expresses the heat necessary to raise the temperature at a constant volume, while the other is a constant multiple of the work done." In particular we infer that this quan-

tity of heat is in itself proportional to the work as soon as the initial temperature is regained, while the pressure and volume may have other values; as pure loss of heat it appears, it is true, only after a complete restoration of the original condition.

Let us now suppose that neither Mariotte's law nor the law of the invariability of the capacities is accurately true. If $c$ and $c'$ are subject to any small changes in consequence of changes of pressure or temperature, we may consider them as functions of $p$ and $v$, and write equations (2) and (3) as follows:—

$$\mathfrak{s} = \frac{\delta}{a\,\alpha}\left(\int cp\,dv + \int \overset{\bullet}{c'}v\,dp\right)$$

$$m\int c'\,d\tau = \frac{\delta}{a\,\alpha}\left(\int c'p\,dv + \int c'v\,dp\right),$$

whence by subtraction,

$$\mathfrak{s} - m\int c'\,d\tau = \frac{\delta}{a\,\alpha}\int (c-c')p\,dv.$$

If now during the change there is only work consumed or work performed, if consequently $p\,dv$ undergoes no change of sign, we may, by a known property of definite integrals, place the factors $c$ and $c-c'$ before the sign of integration, when the last equation will correspond with equation (4): only we now understand by $c'$ and $c-c'$ certain definite mean values between the greatest and least of those which these functions assume during the entire change. The validity of the relation between work and heat is consequently not changed by small variations in the capacities for heat. The proportional number itself is of course subject to simultaneous variations, which however are smaller than the variation in the capacities.

If the gas be restored to its original volume, so that at any time $p\,dv$ must change its sign, the proportional number is no longer necessarily a mean value of $\dfrac{\delta}{a\,\alpha}(c-c')$; yet we see, by representing specially the heat conveyed during positive and negative work, that this number can differ but little from the values of its expression as long as the excess is not too small. If, however, there remains from a large amount of work only a small positive excess, it might be difficult to show that the proportional number could not differ considerably from the values of its expression.

Finally, if Mariotte's law be not strictly accurate, we may put $pv + \rho$ for $pv$, and consider $\rho$ as a small magnitude depending on $p$ and $v$, which at the beginning of the motion is zero. In this case, in place of $p\,dv$ and $v\,dp$, we shall have relatively

$$p\,dv + \frac{d\rho}{dv}\,dv\;;\quad \text{and}\quad v\,dp + \frac{d\rho}{dp}\,dp.$$

The last magnitude is cancelled, and does not occur in the resulting equation. On the other hand, $q$ now becomes

$$q + \int \frac{d\rho}{dv}\,dv.$$

Were ρ of the form $\psi(p) + x(v)$, the quantity added to $q$ would $= \psi(v)$, and would vanish after the restoration to the original volume. In general, however, ρ would produce a change in the quantity of heat, which evidently would be always small, inasmuch as a sudden or quick change in ρ is very improbable.—Poggendorff's *Annalen*, xcvii. p. 30, January 1856; and Silliman's *Journal* for May 1856.

### EXPLOSIVE ACTION OF SODIUM ON WATER.
### BY FREDERICK W. GRIFFIN, PH.D.

The action of sodium on water contained in a glass vessel is such an elegant proof of the composition of that liquid and other points, as to lead to its being occasionally introduced in lectures. I have, however, found the experiment liable to a grave accident, which ought altogether to banish it from public demonstrations. Some years ago, during my private course of lectures, I passed a piece of the metal, about a quarter of an inch square, into a tube filled with water, 18 inches in length by 1 in diameter. When the action was nearly over, a powerful explosion occurred, which forced the tube (weighing upwards of a pound) violently through my hand and dashed it to pieces against the ceiling. As I had often before performed the experiment with perfect safety, I presumed that air must have somehow got mixed with the hydrogen; the more so as, simply holding the sodium in the fingers, I had, to slip it under more quickly, brought the mouth of the tube very near the surface of the water. On all subsequent occasions I placed the sodium in a little tube closed at one end, which it almost filled, and stopping the mouth with the finger, opened it below the larger tube, which was kept at least a couple of inches under water, so that there was no possibility of letting in air by any sudden jerk or otherwise. All went off well for several times, till at a public lecture in Devonshire an explosion resulted more violent than the first, and the tube was blown into splinters which strewed the floor of one half of the room, and slightly wounded several persons. Since that occurrence I have relinquished showing this experiment in public at all, though numerous trials appear to prove that a piece less than a pea may be used with safety, though there is sometimes a slight concussion at the end. The cause of the detonation remains to be explained. In the last instance at any rate, it is quite certain that no explosive mixture with air was formed, and I have little doubt that the effect proceeds from the water round the sodium being thrown into the *spheroidal state*. This view seems confirmed by the fact, that at the first moment of contact a large quantity of gas is always liberated, but the action speedily becomes weaker, and the evolution of hydrogen extremely slow. In all probability the metal is then merely decomposing the atmosphere of aqueous vapour around it; and when the piece is small, it disappears tranquilly in this way; when it is larger, so that the action is prolonged, its temperature slightly falls, contact ensues, and a burst of gas and steam takes place with explosive violence. In both cases the tube was three-quarters full of gas, and

I noticed a sudden downward rush of the liquid the moment before the explosion.

The detonation, with occasional fracture of the vessel, observed by Wagner and Couerbe* to take place when sodium floating fused on water is struck with a spatula, probably proceeds from the same cause, the highly heated globule being forced mechanically into sudden intimate contact with the liquid.

While on the subject of sodium, I may add that when it is melted with a little naphtha in a sealed tube containing no air, it presents to the full extent the high lustre and mobility of mercury, from which indeed it cannot be distinguished by the eye; but as soon as it solidifies, it assumes a slightly crystalline and dead white surface, more nearly resembling frosted silver.

Bristol School of Chemistry,
June 19, 1856.

## METEOROLOGICAL OBSERVATIONS FOR MAY 1856.

*Chiswick.*—May 1. Overcast : cold showers, partly hail : clear and cold. 2. Quite clear : cloudy : frosty at night. 3. Cloudy and cold : showery. 4. Overcast : cloudy : clear, with sharp frost at night. 5. Frosty early A.M. : cloudy and cold. 6. Fine : cloudy and cold : rain. 7. Cold rain. 8. Densely clouded : cold north wind. 9. Heavy clouds. 10. Uniformly overcast : fine. 11. Light haze : fine : cloudy. 12. Uniform haze : rain. 13. Rain : cloudy. 14. Heavy showers, 15. Fine : showers, with some hail. 16. Fine. 17. Cloudy. 18. Boisterous, with rain and hail. 19. Very fine. 20. Very fine : slight frost. 21. Fine : rain. 22. Rain. 23. Cloudy : fine. 24. Fine. 25. Cloudy : rain. 26. Fine. 27. Very fine : heavy rain at night. 28. Cloudy : very fine. 29. Hazy : cloudy : fine. 30. Cloudy and cold. 31. Rain.

Mean temperature of the month ................................. 50°·09
Mean temperature of May 1855 ................................. 48 ·78
Mean temperature of May for the last thirty years ............ 53 ·55
Average amount of rain in May ................................. 1·852 inches.

*Boston.*—May 1. Cloudy : rain and sleet P.M. 2, 3. Cloudy : rain and hail P.M. 4, 5. Cloudy. 6. Cloudy : hail and snow A.M. 7. Cloudy. 8. Cloudy : rain A.M. 9, 10. Cloudy. 11. Fine. 12, 13. Cloudy. 14. Cloudy : rain with thunder P.M. 15, 16. Cloudy. 17. Cloudy : rain P.M. 18. Cloudy : rain A.M. and P.M. 19. Cloudy. 20, 21. Fine. 22. Rain A.M. and P.M. 23. Fine : rain and thunder P.M. 24. Cloudy : rain A.M. and P.M. 25, 26. Cloudy : rain P.M. 27. Fine : rain P.M. 28. Cloudy : rain A.M. and P.M. 29. Cloudy : 30. Fine. 31. Cloudy : rain P.M.

*Sandwick Manse, Orkney.*—May 1. Bright A.M. : cloudy P.M. 2. Sleet-showers A.M. : cloudy P.M. 3—5. Cloudy A.M. and P.M. 6. Cloudy A.M. : clear P.M. 7. Clear A.M. and P.M. 8. Cloudy A.M. : clear P.M. 9. Clear A.M. : rain P.M. 10. Cloudy A.M. : drops P.M. 11. Drizzle A.M. : fog P.M. 12. Hazy A.M. : clear, fine P.M. 13. Cloudy A.M. and P.M. 14. Cloudy A.M. : rain P.M. 15. Cloudy A.M. drops P.M. 16. Bright A.M. : cloudy P.M. 17. Clear, fine A.M. : cloudy, fine P.M. 18. Showers, bright A.M. : showers P.M. 19. Cloudy A.M. : showers P.M. 20. Bright A.M. : clear P.M. 21, 22. Bright A.M. : cloudy P.M. 23, 24. Cloudy A.M. and P.M. 25. Clear A.M. and P.M. 26, 27. Cloudy A.M. and P.M. 28. Bright A.M : cloudy P.M. 29—31. Clear A.M. and P.M.

Mean temperature of May for previous twenty-nine years ... 47°·85
Mean temperature of this month ................................. 46 ·83
Mean temperature of May 1855 ................................. 43 ·81
Average quantity of rain in May for fifteen previous years ... 1·66 inches.

The great drought continues ; the rain during the last three months being less than the average for May alone, which is our driest month, and not half the quantity that fell in March alone last year.

---

* Gmelin, Handbook (Cav. Soc.), vol. iii. p. 75. Berzelius, *Traité*, vol. ii. p. 83.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

| Days of Month. 1856. May. | Barometer. Chiswick. Max. | Min. | Boston. 8¾ a.m. | Orkney, Sandwick. 9¼ a.m. | 8½ p.m. | Thermometer. Chiswick. Max. | Min. | Boston. 8¾ a.m. | Orkney, Sandwick. 9¼ a.m. | 8½ p.m. | Wind. Chiswick. 1 p.m. | Boston. | Orkney, Sandwick. | Rain. Chiswick. | Boston. | Orkney, Sandwick. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. | 29·840 | 29·463 | 29·28 | 29·88 | 30·00 | 47 | 30 | 43·5 | 47 | 42 | ne. | e. | ne. | ·26 | ·10 | |
| 2. | 29·977 | 29·970 | 29·63 | 30·06 | 30·14 | 54 | 27 | 43 | 45 | 40 | nw. | n. | nnw. | ·16 | ·02 | ·02 |
| 3. | 30·098 | 30·015 | 29·68 | 30·22 | 30·30 | 52 | 37 | 40 | 45 | 41 | nw. | n. | nne. | | ·05 | ·02 |
| 4. | 30·111 | 30·048 | 29·76 | 30·26 | 30·22 | 52 | 21 | 43·5 | 44 | 41½ | ne. | nw. | ne. | | ·03 | |
| 5. | 30·172 | 30·044 | 29·75 | 30·16 | 30·11 | 53 | 35 | 43·5 | 45 | 40 | se. | e. | ne. | ·24 | | |
| 6. | 29·917 | 29·568 | 29·60 | 29·99 | 29·94 | 52 | 41 | 47 | 45½ | 36½ | n. | ne. | n. | ·16 | ·06 | |
| 7. | 29·609 | 29·409 | 29·20 | 29·92 | 30·07 | 44 | 39 | 50 | 48 | 41½ | n. | n. | e. | | | |
| 8. | 30·115 | 29·792 | 29·52 | 30·22 | 30·30 | 49 | 41 | 45 | 44½ | 42½ | ne. | ne. | se. | | ·03 | ·02 |
| 9. | 30·151 | 30·099 | 29·82 | 30·23 | 30·08 | 54 | 43 | 46 | 49½ | 47 | ne. | ne. | ese. | | | |
| 10. | 30·015 | 29·898 | 29·71 | 30·01 | 29·97 | 68 | 44 | 50 | 53 | 48½ | n. | ne. | nw. | ·70 | | ·31 |
| ☽ 11. | 29·870 | 29·777 | 29·48 | 29·93 | 29·93 | 71 | 43 | 52·5 | 49 | 50 | ne. | ne. | e. | ·14 | | ·07 |
| 12. | 29·719 | 29·637 | 29·36 | 29·91 | 29·87 | 64 | 52 | 52·5 | 50 | 46 | sw. | s. | e. | ·19 | | ·07 |
| 13. | 29·628 | 29·606 | 29·22 | 29·70 | 29·62 | 65 | 40 | 55 | 52·5 | 49½ | sw. | s. | nne. | ·20 | | |
| 14. | 29·543 | 29·521 | 29·10 | 29·62 | 29·61 | 58 | 40 | 58 | 51 | 46 | sw. | n. | n. | | | ·04 |
| 15. | 29·492 | 29·394 | 29·08 | 29·53 | 29·48 | 62 | 40 | 55 | 47½ | 45 | sw. | sw. | w. | | | ·10 |
| 16. | 29·610 | 29·447 | 29·06 | 29·40 | 29·44 | 64 | 41 | 52·5 | 48½ | 46 | sw. | s. | e. | ·34 | ·21 | |
| 17. | 29·651 | 29·472 | 29·18 | 29·44 | 29·36 | 62 | 40 | 61 | 50 | 47 | sw. | sw. | ene. | ·13 | | ·12 |
| 18. | 29·518 | 29·440 | 28·96 | 29·33 | 29·42 | 60 | 40 | 50 | 52 | 45 | sw. | s. | nnw. | | | |
| ○ 19. | 29·996 | 29·702 | 29·21 | 29·62 | 29·81 | 70 | 30 | 51·5 | 47½ | 44 | sw. | w. | nw. | | ·06 | ·05 |
| 20. | 30·058 | 30·019 | 29·60 | 29·91 | 30·00 | 71 | 30 | 53 | 46½ | 45½ | sw. | whw. | e. | | ·23 | ·02 |
| 21. | 29·937 | 29·746 | 29·50 | 29·97 | 29·94 | 73 | 50 | 59 | 49½ | 47 | s. | s. | e. | ·24 | | |
| 22. | 29·634 | 29·586 | 29·20 | 29·83 | 29·76 | 64 | 36 | 55 | 49 | 45½ | sw. | s. | ne. | ·16 | ·15 | |
| 23. | 29·576 | 29·482 | 29·10 | 29·65 | 29·64 | 66 | 42 | 62 | 48 | 48 | s. | sw. | e. | ·04 | ·18 | ·04 |
| 24. | 29·549 | 29·475 | 29·04 | 29·67 | 29·73 | 63 | 42 | 65 | 49½ | 49 | sw. | sw. | ese. | | ·20 | ·10 |
| 25. | 29·807 | 29·491 | 29·15 | 29·74 | 29·80 | 64 | 44 | 61·5 | 51½ | 50 | sw. | sse. | ese. | ·14 | ·21 | ·12 |
| 26. | 29·887 | 29·777 | 29·37 | 29·78 | 29·73 | 70 | 48 | 60 | 50 | 45 | sw. | sse. | n. | ·02 | ·07 | |
| ☾ 27. | 29·801 | 29·626 | 29·30 | 29·69 | 29·69 | 70 | 47 | 58·5 | 47½ | 47 | sw. | w. | n. | ·63 | ·09 | ·02 |
| 28. | 29·708 | 29·583 | 29·11 | 29·72 | 29·84 | 69 | 40 | 50 | 53½ | 46 | nw. | s. | ese. | ·41 | ·31 | |
| 29. | 30·005 | 29·853 | 29·48 | 30·00 | 30·16 | 60 | 36 | 53·5 | 48½ | 44 | ne. | ne. | ese. | | ·20 | ·03 |
| 30. | 30·130 | 30·084 | 29·75 | 30·17 | 30·15 | 57 | 41 | 48·5 | 52 | 48 | n. | ne. | | | | |
| 31. | 29·880 | 29·700 | 29·53 | 30·08 | 30·02 | 51 | 47 | | 50½ | 47½ | | | | ·22 | | |
| Mean. | 29·838 | 29·700a | 29·38 | 29·859 | 29·880 | 60·61 | 39·58 | 52·2 | 48·72 | 44·95 | | | | 4·38 | 2·20 | 0·89 |

X. *On a modified Form of the second Fundamental Theorem in the Mechanical Theory of Heat.* By R. CLAUSIUS*.

IN my memoir "On the Moving Force of Heat, and the Laws which can be deduced therefrom †," I have shown that the theorem of the equivalence of heat and work, and Carnot's theorem, are not mutually exclusive, but that, by a small modification of the latter, which does not affect its principal part, they can be brought into accordance. With the exception of this indispensable change, I allowed the theorem of Carnot to retain its original form, my chief object then being, by the application of the two theorems to special cases, to arrive at conclusions which, according as they involved known or unknown properties of bodies, might suitably serve as proofs of the truth of the theorems, or as examples of their fecundity.

This form, however, although it may suffice for the deduction of the equations which depend upon the theorem, is incomplete, because we cannot recognize therein, with sufficient clearness, the real nature of the theorem, and its connexion with the first fundamental theorem. The modified form in the following pages will, I think, better fulfil this demand, and in its applications will be found very convenient.

Before proceeding to the examination of the second theorem, I may be allowed a few remarks on the first theorem, so far as this is necessary for the supervision of the whole. It is true that I

---

* The present memoir appeared in Poggendorff's *Annalen,* vol. xciii. p. 481, and was referred to by the author in a letter lately published in this Magazine; it is also employed to a considerable extent in a memoir on the steam-engine by the same author, a translation of which will shortly appear.

† Phil. Mag. vol. ii. pp. 1, 102.

might assume this as known from my former memoirs or from those of other authors, but to refer back would be inconvenient; and besides this, the demonstration I shall here give is preferable to my former one, because it is at once more general and more concise.

### Theorem of the equivalence of Heat and Work.

Whenever a moving force generated by heat acts against another force, and motion in the one direction or the other ensues, positive work is performed by the one force at the same time that negative work is done by the other. As this work has only to be considered as a simple quantity in calculation, it is perfectly arbitrary, in determining its sign, which of the two forces is chosen as the indicator. In researches which have a special reference to the moving force of heat it is customary to determine the sign accordingly, by counting as positive the work done by heat in overcoming any other force, and as negative the work done by such other force. In this manner the theorem of the equivalence of heat and work, which forms only a particular case of the general relation between *vis viva* and mechanical work, can be briefly enunciated thus :—

*Mechanical work may be transformed into heat, and conversely heat into work, the magnitude of the one being always proportional to that of the other.*

The forces which here enter into consideration may be divided into two classes : those which the atoms of a body exert upon each other, and which depend, of course, upon the nature of the body, and those which arise from the foreign influences to which the body may be exposed. According to these two classes of forces which have to be overcome, I have divided the work done by heat into *internal* and *external* work, which are subjected to essentially different laws.

With respect to the internal work, it is easy to see that when a body, departing from its initial condition, suffers a series of modifications and ultimately returns to its original state, the quantities of internal work thereby produced must exactly cancel one another. For if any positive or negative quantity of internal work had remained, it must have produced an opposite external quantity of work or a change in the existing quantity of heat, and as the same process could be repeated any number of times, it would be possible, according to the sign, either to produce work or heat continually from nothing, or else to lose work or heat continually, without obtaining any equivalent; both of which cases are universally allowed to be impossible. But if at every return of the body to its initial condition the quantity of internal work is zero, it follows, further, that the internal work

corresponding to any given change in the condition of the body is completely determined by the initial and final conditions of the latter, and is independent of the path pursued in passing from one to the other. Conceive a body to pass successively in different ways from the one condition to the other, but always to return in the same manner to its initial state. It is evident that the quantities of internal work produced along the different paths must all cancel with the common quantity produced during the return, and consequently must be equal to each other.

It is otherwise with the external work. With the same initial and final conditions, this can vary just as much as the external influences to which the body may be exposed can differ.

Let us now consider at once the internal and external work produced during any given change of condition. If opposite in sign they may partially cancel each other, and what remains must then be proportional to the simultaneous change which has occurred in the quantity of heat. In calculation, however, it amounts to the same thing if we assume an alteration in the quantity of heat equivalent to each of the two kinds of work. Let Q, therefore, be the quantity of heat which must be imparted to a body during its passage, in a given manner, from one condition to another, any heat withdrawn from the body being counted as an imparted negative quantity of heat. Then Q may be divided into three parts, of which the first is employed in increasing the heat actually existing in the body, the second in producing the internal, and the third in producing the external work. What was before stated of the second part also applies to the first—it is independent of the path pursued in the passage of the body from one state to another: hence both parts together may be represented by one function U, of which we at present only know that it is completely determined by the initial and final states of the body. The third part, however, the equivalent of external work, can, like this work itself, only be determined when the precise manner in which the changes of condition took place is known. If W be the quantity of external work, and A the equivalent of heat for the unit of work, the value of the third part will be A.W, and the first fundamental theorem will be expressed by the equation

$$Q = U + A \cdot W. \quad \cdots \cdots \quad (I)$$

When the series of changes are of such a nature that through them the body returns to its original condition, or when, as we shall in future express it, these changes form a *circular process,* we have
$$U = 0,$$
and the foregoing equation becomes
$$Q = A \cdot W. \quad \cdots \cdots \cdots \quad (1)$$

In order to give special forms to equation (I), in which it shall express definite properties of bodies, we must make special assumptions with respect to the foreign influences to which the body is exposed. For instance, we will assume that the only active external force, or at least the only one requiring consideration in the determination of work, is an external pressure everywhere normal to the surface, and equally intense at every point of the same, which is always the case with liquid and gaseous bodies when other foreign forces are absent, and might at least be the case with solid bodies. It will be seen that under this condition it is not necessary, in determining the external work, to consider the variations in form experienced by the body, and its expansion or contraction in different directions, but only the total change in its volume. We will further assume that the pressure always changes very gradually, so that at any moment it shall differ so little from the opposite expansive force of the body, that both may be counted as equal. Thus the pressure constitutes a property of the body itself, which can be determined from its other contemporaneous properties.

In general, under the above circumstances, we may consider the pressure as well as the condition of the body, so far as it is essential to us, as determined so soon as its temperature $t$ and volume $v$ are given. We shall make these two magnitudes, therefore, our independent variables, and shall consider the pressure $p$ as well as the quantity U in the equation (I) as functions of these. If, now, $t$ and $v$ receive the increments $dt$ and $dv$, the corresponding quantity of external work done can be easily ascertained. If any increase of temperature is not accompanied by a change of volume, no external work is produced; on the other hand, if, with respect to the differentials, we neglect terms higher than the second in order, then the work done during an increment of volume $dv$ will be $pdv$. Hence the work done during a simultaneous increase of $t$ and $v$ is

$$d\,W = pdv,$$

and when we apply this to the equation (I), we obtain

$$dQ = dU + A \cdot pdv. \qquad . \quad . \quad . \quad . \quad (2)$$

On account of the term $A \cdot pdv$, this equation can only be integrated as soon as we have a relation between $t$ and $v$, by means of which $t$ and $p$ may be expressed as functions of $v$ alone. It is this relation which, as above required, defines the manner in which the changes of condition take place.

The unknown function U may be eliminated from this equation. When written in the form

$$\frac{dQ}{dt}\,dt + \frac{dQ}{dv}\,dv = \frac{dU}{dt}\,dt + \left(\frac{dU}{dv} + A \cdot p\right)dv,$$

we easily see that it is divisible into the two equations

$$\frac{d\mathrm{Q}}{dt} = \frac{d\mathrm{U}}{dt},$$

and

$$\frac{d\mathrm{Q}}{dv} = \frac{d\mathrm{U}}{dv} + \mathrm{A} \cdot p.$$

Let the first of these be differentiated according to $v$ and the second according to $t$. In doing so we may apply to U the well-known theorem, that when a function of two independent variables is successively differentiated according to both, the order in which this is done does not affect the result. This theorem, however, does not apply to the magnitude Q, and we must use symbols which will show the order of differentiation. This is done in the following equations :—

$$\frac{d}{dv}\left(\frac{d\mathrm{Q}}{dt}\right) = \frac{d^2\mathrm{U}}{dt \cdot dv},$$

$$\frac{d}{dt}\left(\frac{d\mathrm{Q}}{dv}\right) = \frac{d^2\mathrm{U}}{dt\,dv} + \mathrm{A} \cdot \frac{dp}{dt}.$$

By subtraction, we have

$$\frac{d}{dt}\left(\frac{d\mathrm{Q}}{dv}\right) - \frac{d}{dv}\left(\frac{d\mathrm{Q}}{dt}\right) = \mathrm{A} \cdot \frac{dp}{dt}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

an equation which no longer contains U.

The equations (2) and (3) can be still further specialized by applying them to particular classes of bodies. In my former memoir I have shown these special applications in two of the most important cases, viz. permanent gases and vapours at a maximum density. On this account I will not here pursue the subject further, but pass on to the consideration of the second fundamental theorem in the mechanical theory of heat.

## *Theorem of the equivalence of transformations.*

Carnot's theorem, when brought into agreement with the first fundamental theorem, expresses a relation between two kinds of transformations, the transformation of heat into work, and the passage of heat from a warmer to a colder body, which may be regarded as the transformation of heat at a higher into heat at a lower temperature. In its original form it may be enunciated in some such manner as the following :—*In all cases where a quantity of heat is converted into work, and where the body effecting this transformation ultimately returns to its original condition, another quantity of heat must necessarily be transferred from a warmer to a colder body ; and the magnitude of the last quantity of heat, in relation to the first, depends only upon the temperatures*

*of the bodies between which heat passes, and not upon the nature of the body effecting the transformation.*

The demonstration of this theorem, however, is based upon too simple a process, in which only two bodies losing or receiving heat are employed, and where it is tacitly assumed that one of the two bodies between which the transmission of heat takes place is the source of the heat which is converted into work. But in this manner, by previously assuming a particular temperature for the heat converted into work, the influence which a change of this temperature has upon the relation between the two quantities of heat remains concealed, and therefore the theorem in the above form is incomplete.

It is true this influence may be determined without great difficulty by combining the theorem in the above limited form with the first fundamental theorem, and thus completing the former by the adding to it the results thus arrived at. But by this indirect method the whole subject would lose much of its clearness and facility of supervision, and on this account it appears to me preferable to deduce the general form of the theorem immediately from the same principles which I have already employed in my former memoir, in order to prove the modified theorem of Carnot.

This principle, upon which the whole of the following development rests, is as follows :—*Heat can never pass from a colder to a warmer body without some other change, connected therewith, occurring at the same time.* Everything we know concerning the interchange of heat between two bodies of different temperatures confirms this, for heat everywhere manifests a tendency to equalize existing differences of temperature, and therefore to pass in a contrary direction, *i. e.* from warmer to colder bodies. Without further explanation, therefore, the truth of the principle will be granted.

For the present we will again use the well-known process first conceived by Carnot and graphically represented by Clapeyron, with this difference, however, that, besides the two bodies between which the transmission of heat takes place, we shall assume a third, at any temperature, which shall furnish the heat converted into work. An example being the only thing now required, we shall choose as the changing body one whose changes are governed by the simplest possible laws, *e. g.* a permanent gas. Let, therefore, a quantity of permanent gas having the temperature $t$ and volume $v$ be given. In the adjoining figure we shall suppose the volume represented by the abscissa $o\,h$, and the pressure exerted by the gas at this volume, and at the temperature $t$, by the ordinate $h\,a$. This gas we subject, successively, to the following operations :—

1. The temperature $t$ of the gas is changed to $t_1$, which, for
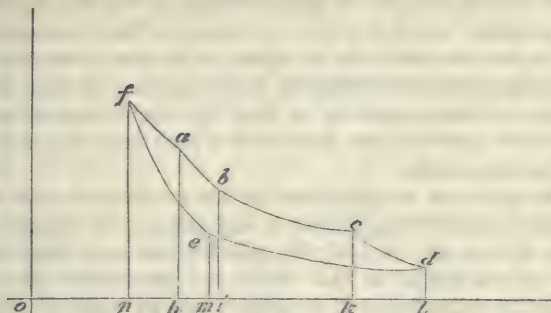


the sake of an example, may be less than $t$. To do this, the gas may be enclosed within a surface impenetrable to heat, and allowed to expand without either receiving or losing heat. The diminution of pressure, consequent upon the simultaneous increase of volume and decrease of temperature, is represented by the curve $a\,b$; so that, when the temperature of the gas has reached $t_1$, its volume and pressure have become $o\,i$ and $i\,b$ respectively.

2. The gas is next placed in communication with a body $K_1$, of the temperature $t_1$, and allowed to expand still more, in such a manner, however, that all the heat lost by expansion is again supplied by the body. With respect to this body, we shall assume that, owing to its magnitude or to some other cause, its temperature does not become appreciably lower by this expenditure of heat, and therefore that it may be considered constant. Consequently, during expansion the gas will also preserve a constant temperature, and the diminution of the pressure will be represented by a portion of an equilateral hyperbola $b\,c$. The quantity of heat furnished by $K_1$ shall be $Q_1$.

3. The gas is now separated from the body $K_1$ and allowed to expand still further, but without receiving or losing heat, until its temperature has diminished from $t_1$ to $t_2$. The consequent diminution of pressure is represented by the curve $c\,d$, which is of the same nature as $a\,b$.

4. The gas is now put in communication with a body $K_2$, having the constant temperature $t_2$, and compressed; all the heat thus produced in it being imparted to $K_2$. This compression is continued until $K_2$ has received the same quantity of heat $Q_1$ as was before furnished by $K_1$. The pressure will increase according to the equilateral hyperbola $d\,e$.

5. The gas is then separated from the body $K_2$ and compressed, without being permitted to receive or lose heat, until

its temperature rises from $t_2$ to its original value $t$, the pressure increasing according to the curve $ef$. The volume $on$ to which the gas is thus reduced is smaller than its original volume $oh$, for the pressure which had to be overcome in the compression $de$, and therefore the work to be spent, were less than the corresponding magnitudes during the expansion $bc$; so that, in order to restore the same quantity of heat $Q_1$, the compression must be continued further than would have been necessary merely to annul the expansions.

6. The gas is at length placed in communication with a body K, of the constant temperature $t$, and allowed to expand to its original volume $oh$, the body K replacing the heat thus lost, the amount of which may be Q. When the gas reaches the volume $oh$ with the temperature $t$, it must exert its original pressure, and the equilateral hyperbola, which represents the last diminution of pressure, will precisely meet the point $a$.

These six changes together constitute a *circular process*, the gas ultimately returning to its original condition. Of the three bodies K, $K_1$ and $K_2$, which throughout the whole process are considered merely as sources or reservoirs of heat, the two first have lost the quantities of heat Q and $Q_1$, and the third has received the quantity $Q_1$, or, as we may express it, $Q_1$ has been transferred from $K_1$ to $K_2$, and Q has disappeared. The last quantity of heat must, according to the first theorem, have been converted into external work. The pressure of the gas during expansion being greater than during compression, and therefore the positive amount of work greater than the negative, there has been a gain of external work, which is evidently represented by the area of the closed figure $abcdef$. If we call this amount of work W, then, according to equation (I),

$$Q = A \cdot W.$$

The whole of the above-described circular process may be reversed or executed in an opposite manner by connecting the gas with the same bodies and under the same circumstances as before, executing the reverse operations, *i. e.* commencing with the compression $af$, after which would follow the expansions $fe$ and $ed$, and lastly the compressions $dc$, $cb$ and $ba$. The bodies K and $K_1$ will now evidently *receive* the quantities of heat Q and $Q_1$, and $K_2$ will *lose* the quantity $Q_1$. At the same time the negative work is now greater than the positive, so that the area of the closed figure now represents a *loss* of work. The result of the reverse process, therefore, is that the quantity of heat $Q_1$ has been transferred from $K_2$ to $K_1$, and the quantity of heat Q, generated from work, given to the body K.

In order to learn the mutual dependence of the two simulta-

neous transformations above described, we shall first assume that the temperatures of the three reservoirs of heat remain the same, but that the circular processes through which the transformations are effected are different. This will be the case when, instead of a gas, some other body is submitted to similar transformations, or when the circular processes are of any other kind, subject only to the conditions that the three bodies K, $K_1$ and $K_2$ are the only ones which receive or impart heat, and of the two latter the one receives as much as the other loses. These several processes can be either reversible, as in the foregoing case, or not, and the law which governs the transformations will vary accordingly. Nevertheless, the modification which the law for non-reversible processes suffers may be easily applied afterwards, so that at present we will confine ourselves to the consideration of *reversible* circular processes.

With respect to all these it may be proved from the foregoing principle, that the quantity of heat $Q_1$, transferred from $K_1$ to $K_2$, has always the same relation to Q, the quantity of heat transformed into work. For if there were two such processes wherein, Q being the same, $Q_1$ was different, then the two processes could be executed successively, the one in which $Q_1$ was smaller in a direct, the other in an opposite manner. Then the quantity of heat Q, which by the first process was converted into work, would be again transformed into heat by the second process and restored to the body K, and in other respects everything would ultimately return to its original condition; with this sole exception, however, that more heat would have passed from $K_2$ to $K_1$ than in the opposite direction. On the whole, therefore, a transmission of heat from a colder body $K_2$ to a warmer $K_1$ has occurred, which, in contradiction to the principle before mentioned, has not been compensated in any manner.

Of the two transformations in such a reversible process either can replace the other, if the latter is taken in an opposite direction; so that if a transformation of the one kind has occurred, this can be again reversed, and a transformation of the other kind may be substituted without any other permanent change being requisite thereto. For example, let the quantity of heat Q, produced in any manner whatever from work, be received by the body K; then by the foregoing circular process it can be again withdrawn from K and transformed back into work, but at the same time the quantity of heat $Q_1$ will pass from $K_1$ to $K_2$; or if the quantity of heat $Q_1$ had previously been transferred from $K_1$ to $K_2$, this can be again restored to $K_1$ by the transformation of work into the quantity of heat Q of the temperature of the body K.

We see, therefore, that these two transformations may be

regarded as phænomena of the same nature, and we may call two transformations which can thus mutually replace one another *equivalent*. We have now to find the law according to which the transformations must be expressed as mathematical magnitudes, in order that the equivalence of two transformations may be evident from the equality of their values. The mathematical value of a transformation thus determined may be called its *equivalence-value* (Aequivalenzwerth).

With respect to the direction in which each transformation is to be considered positive, it may be chosen arbitrarily in the one, but it will then be fixed in the other, for it is clear that the transformation which is equivalent to a positive transformation must itself be positive. In future we shall consider *the transformation from work to heat as positive, and therefore the transmission of heat from a higher to a lower temperature will be also positive.*

With respect to the magnitude of the equivalence-value, it is first of all clear that the value of a transformation from work into heat must be proportional to the quantity of heat produced, and besides this it can only depend upon the temperature. Hence the equivalence-value of the transformation of the quantity of heat $Q$, of the temperature $t$, from work, may be represented generally by

$$Q \cdot f(t),$$

wherein $f(t)$ is a function of the temperature, which is the same for all cases. When $Q$ is negative in this formula, it will indicate that the quantity of heat $Q$ is transformed, not from work into heat, but from heat into work. In a similar manner the value of the transmission of the quantity of heat $Q$, from the temperature $t_1$ to the temperature $t_2$, must be proportional to the quantity transmitted, and besides this, can only depend upon the two temperatures. In general, therefore, it may be expressed by

$$Q \cdot F(t_1, t_2),$$

wherein $F(t_1, t_2)$ is a function of both temperatures, which is the same for all cases, and of which we at present only know that, without changing its numerical value, it must change its sign when the two temperatures are interchanged; so that

$$F(t_2, t_1) = -F(t_1, t_2). \quad . \quad . \quad . \quad . \quad (4)$$

In order to institute a relation between these two expressions, we have the condition, that in every reversible circular process of the above kind, the two transformations which are involved must be equal in magnitude, but opposite in sign; so that their algebraical sum must be zero. For instance, in the process for a gas, so fully described above, the quantity of heat $Q$, at the

temperature $t$, was converted into work; this gives $-Q \cdot f(t)$ as its equivalence-value, and that of the quantity of heat $Q_1$, transferred from the temperature $t_1$ to $t_2$, will be $Q_1 F(t_1, t_2)$, so that we have the equation

$$-Q \cdot f(t) + Q_1 \cdot F(t_1, t_2) = 0. \quad . \quad . \quad . \quad . \quad (5)$$

Let us now conceive a similar process executed in an opposite manner, so that the bodies $K_1$ and $K_2$, and the quantity of heat $Q_1$, passing between them, remain the same as before; but that instead of the body K of the temperature $t$, another body K′ of the temperature $t'$ be employed; and let us call the quantity of heat produced by work in this case $Q'$,—then, analogous to the last, we shall have the equation

$$Q'f(t') + Q_1 F(t_2, t_1) = 0. \quad . \quad . \quad . \quad . \quad (6)$$

Adding these two equations, and applying (4), we have

$$-Q f(t) + Q' f(t') = 0. \quad . \quad . \quad . \quad . \quad (7)$$

If now we regard these two circular processes together as one circular process, which is of course allowable, then in the latter the transmissions of heat between $K_1$ and $K_2$ will no longer enter into consideration, for they precisely cancel one another, and there remain only the quantity of heat Q taken from K and transformed into work, and the quantity Q′ generated by work and given to K′. These two transformations of the *same* kind, however, may be so divided and combined as again to appear as transformations of different kinds. If we hold simply to the fact that a body K has lost the quantity of heat Q, and another body K′ has received the quantity Q′, we may without hesitation consider the part common to both as transferred from K to K′, and regard only the other part, the excess of one quantity over the other, as a transformation from work into heat, or *vice versâ*. For example, let the temperature $t'$ be greater than $t$, so that the above, being a transmission from the colder to the warmer body, will be negative. Then the other transformation must be positive, that is, a transformation from work into heat, whence it follows that the quantity of heat Q′ imparted to K′ must be greater than the quantity Q lost by K. If we divide Q′ into the two parts

$$Q \text{ and } Q' - Q,$$

the first will be the quantity of heat transferred from K to $K_1$, and the second the quantity generated from work.

According to this view the double process appears as a process of the same kind as the two simple ones of which it consists, for the circumstance that the generated heat is not imparted to a third body, but to one of the two between which the transmission of heat takes place, makes no essential

difference, because the temperature of the generated heat is arbitrary, and may therefore have the same value as the temperature of one of the two bodies, in which case a third body would be superfluous. Consequently, for the two quantities of heat Q and $Q' - Q$, an equation of the same form as (6) must hold, *i. e.*

$$(Q' - Q)f(t') + QF(t, t') = 0.$$

Eliminating the magnitude $Q'$ by means of (7), and dividing by Q, this equation becomes

$$F(t, t') = f(t') - f(t), \quad . \quad . \quad , \quad . \quad . \quad (8)$$

so that the temperatures $t$ and $t'$ being arbitrary, the function of two temperatures which applies to the second kind of transformation is reduced, in a general manner, to the function of one temperature which applies to the first kind.

For brevity, we will introduce a simpler symbol for the last function, or rather for its reciprocal, inasmuch as the latter will afterwards be shown to be the more convenient of the two. Let us therefore make

$$f(t) = \frac{1}{T}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

so that T is now the unknown function of the temperature involved in the equivalence-values. Further, $T_1$, $T_2$, &c. shall represent particular values of this function, corresponding to the temperatures $t_1$, $t_2$, &c.

According to this, the second fundamental theorem in the mechanical theory of heat, which in this form might appropriately be called the *theorem of the equivalence of transformations,* may be thus enunciated:

*If two transformations which, without necessitating any other permanent change, can mutually replace one another, be called equivalent, then the generation of the quantity of heat* Q *of the temperature* t *from work, has the equivalence-value*

$$\frac{Q}{T},$$

*and the passage of the quantity of heat* Q *from the temperature* $t_1$ *to the temperature* $t_2$, *has the value*

$$Q\left(\frac{1}{T_2} - \frac{1}{T_1}\right),$$

*wherein* T *is a function of the temperature, independent of the nature of the process by which the transformation is effected.*

If, to the last expression, we give the form

$$\frac{Q}{T_2} - \frac{Q}{T_1},$$

it is evident that the passage of the quantity of heat Q, from the temperature $t_1$ to the temperature $t_2$, has the same equivalence-value as a double transformation of the first kind, that is to say, the transformation of the quantity Q from heat, at the temperature $t_1$ into work, and from work into heat at the temperature $t_2$. A discussion of the question how far this external agreement is based upon the nature of the process itself would be out of place here; but at all events, in the mathematical determination of the equivalence-value, every transmission of heat, no matter how effected, can be considered as such a combination of two opposite transformations of the first kind.

By means of this rule, it will be easy to find a mathematical expression for the total value of all the transformations of both kinds, which are included in any circular process, however complicated. For instead of examining what part of a given quantity of heat received by a reservoir of heat, during the circular process, has arisen from work, and whence the other part has come, every such quantity received may be brought into calculation as if it had been generated by work, and every quantity lost by a reservoir of heat, as if it had been converted into work. Let us assume that the several bodies $K_1$, $K_2$, $K_3$, &c., serving as reservoirs of heat at the temperatures $t_1$, $t_2$, $t_3$, &c., have received during the process the quantities of heat $Q_1$, $Q_2$, $Q_3$, &c., whereby the loss of a quantity of heat will be counted as the gain of a negative quantity of heat; then the total value N of all the transformations will be

$$N = \frac{Q_1}{T_1} + \frac{Q_2}{T_2} + \frac{Q_3}{T_3} + \&c. . = \Sigma \frac{Q}{T}.. \quad . \quad . \quad . \quad (10)$$

It is here assumed that the temperatures of the bodies $K_1$, $K_2$, $K_3$, &c. are constant, or at least so nearly constant, that their variations may be neglected. When one of the bodies, however, either by the reception of the quantity of heat Q itself, or through some other cause, changes its temperature during the process so considerably, that the variation demands consideration, then for each element of heat $dQ$ we must employ that temperature which the body possessed at the time it received it, whereby an integration will be necessary. For the sake of generality, let us assume that this is the case with all the bodies; then the foregoing equation will assume the form

$$N = \int \frac{dQ}{T}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

wherein the integral extends over all the quantities of heat received by the several bodies.

If the process is *reversible*, then, however complicated it may be, we can prove, as in the simple process before considered,

*that the transformations which occur must exactly cancel each other, so that their algebraical sum is zero.*

For were this not the case, then we might conceive all the transformations divided into two parts, of which the first gives the algebraical sum zero, and the second consists entirely of transformations having the same sign.  By means of a finite or infinite number of simple circular processes, the transformations of the first part must admit of being made in an opposite manner, so that the transformations of the second part would alone remain without any other change.  Were these transformations *negative*, *i.e.* from heat into work, and the transmission of heat from a lower to a higher temperature, then of the two the first could be replaced by transformations of the latter kind, and ultimately transmissions of heat from a lower to a higher temperature would alone remain, which would be compensated by nothing, and therefore contrary to the above principle. Further, were those transformations *positive*, it would only be necessary to execute the operations in an inverse manner to render them negative, and thus obtain the foregoing impossible case again.  Hence we conclude that the second part of the transformations can have no existence.

Consequently the equation

$$\int \frac{d\mathrm{Q}}{\mathrm{T}} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(II)}$$

is the analytical expression of the second fundamental theorem in the mechanical theory of heat.

The application of this equation can be considerably extended by giving to the magnitude $t$ involved in it a somewhat different signification.  For this purpose, let us consider a circular process consisting of a series of changes of condition made by a body which ultimately returns to its original state, and for simplicity, let us assume that all parts of the body have the same temperature; then in order that the process may be reversible, the changing body when imparting or receiving heat can only be placed in communication with such bodies as have the same temperature as itself, for only in this case can the heat pass in an opposite direction.  Strictly speaking, this condition can never be fulfilled if a motion of heat at all occurs; but we may assume it to be so nearly fulfilled, that the small differences of temperature still existing may be neglected in the calculation. In this case it is of course of no importance whether $t$, in the equation (II), represents the temperature of the reservoir of heat just employed, or the momentary temperature of the changing body, inasmuch as both are equal.  The latter signification being once adopted, however, it is easy to see that any

other temperatures may be attributed to the reservoirs of heat without producing thereby any change in the expression $\int \frac{d\mathrm{Q}}{\mathrm{T}}$ which shall be prejudicial to the validity of the foregoing equation. As with this signification of $t$ the several reservoirs of heat need no longer enter into consideration, it is customary to refer the quantities of heat, not to them, but to the changing body itself, by stating what quantities of heat this body successively receives or imparts during its modifications. If hereby a quantity of heat received be again counted as positive, and a quantity imparted as negative, all quantities of heat will of course be affected with a sign opposite to that which was given to them with reference to the reservoirs of heat, for every quantity of heat *received* by the changing body is *imparted* to it by some reservoir of heat; nevertheless, this circumstance can have no influence upon the equation which expresses that the value of the whole integral is zero. From what has just been said, it follows, therefore, that when for every quantity of heat $d\mathrm{Q}$ which the body receives or, if negative, imparts during its modifications the temperature of the body at the moment be taken into calculation, the equation (II) may be applied without further considering whence the heat comes or whither it goes, provided always that the process is reversible.

To the equation (II) thus interpreted we can now give a more special form, as was formerly done to equation (I), in which form it shall express a particular property of the body. We shall thus obtain an equation essentially the same as the well-known one deduced by Clapeyron from the theorem of Carnot*. With respect to the nature of the modifications, we shall assume the same conditions as before led to the deduction of the equations (2) and (3) from (I), and which also suffice for the fulfilment of equation (II). Hence, the condition of the body being defined by its temperature $t$ and volume $v$, we have

$$d\mathrm{Q} = \frac{d\mathrm{Q}}{dt}\,dt + \frac{d\mathrm{Q}}{dv}\,dv.$$

Inasmuch as by (II) $\int \frac{d\mathrm{Q}}{\mathrm{T}}$ must always equal zero, whenever $t$ and $v$ assume their initial values, the expression under the integral sign, which by the foregoing equation becomes

$$\frac{1}{\mathrm{T}} \cdot \frac{d\mathrm{Q}}{dt}\,dt + \frac{1}{\mathrm{T}} \cdot \frac{d\mathrm{Q}}{dv}\,dv,$$

must be a complete differential, if $t$ and $v$ are independent variables; and the two terms of the expression must consequently

* *Journ. de l'Ecole Polytechnique,* tome xix.

satisfy the following condition,

$$\frac{d}{dt}\left(\frac{1}{T}\cdot\frac{dQ}{dv}\right)=\frac{d}{dv}\left(\frac{1}{T}\cdot\frac{dQ}{dt}\right).$$

From this we obtain

$$\frac{1}{T}\cdot\frac{d}{dt}\left(\frac{dQ}{dv}\right)-\frac{dQ}{dv}\cdot\frac{\dfrac{dT}{dt}}{T^2}=\frac{1}{T}\cdot\frac{d}{dv}\left(\frac{dQ}{dt}\right)$$

or

$$\frac{dQ}{dv}\cdot\frac{dT}{dt}=T\cdot\left[\frac{d}{dt}\left(\frac{dQ}{dv}\right)-\frac{d}{dv}\left(\frac{dQ}{dt}\right)\right]. \quad . \quad . \quad . \quad (12)$$

Substituting, from equation (3), the value of the expression within the [ ], we obtain the desired equation,

$$\frac{dQ}{dv}\cdot\frac{dT}{dt}=A\cdot T\frac{dp}{dt}, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

which, on account of the relation

$$\frac{dp}{dt}=\frac{dp}{dT}\cdot\frac{dT}{dt},$$

may be written thus:

$$\frac{dQ}{dv}=A\cdot T\frac{dp}{dT}. \quad . \quad . \quad . \quad . \quad (13\,a)$$

If we compare this result with the before-mentioned equation established by Clapeyron, we shall at once see the relation which exists between the function T, here introduced, and that used by Clapeyron, denoted by C, and known as Carnot's function, which I have also used in former memoirs. This relation may be expressed thus:

$$\frac{\dfrac{dT}{dt}}{T}=\frac{A}{C}. \quad . \quad . \quad . \quad . \quad . \quad (14)$$

We proceed now to the consideration of *non-reversible* circular processes.

In the proof of the previous theorem, that in any compound reversible process the algebraical sum of all the transformations must be zero, it was first shown that the sum could not be *negative*, and afterwards that it could not be *positive*, for if so it would only be necessary to reverse the process in order to obtain a negative sum. The first part of this proof remains unchanged even when the process is not reversible; the second part, however, cannot be applied in such a case. Hence we obtain the following theorem, which applies generally to all circular processes, those that are reversible forming the limit :—

*The algebraical sum of all transformations occurring in a circular process can only be positive,*

A transformation which thus remains at the conclusion of a circular process without another opposite one, and which according to this theorem can only be positive, we shall, for brevity, call an *uncompensated* transformation.

The different kinds of operations giving rise to uncompensated transformations are, as far as external appearances are concerned, rather numerous, even though they may not differ very essentially. One of the most frequently occurring examples is that of the transmission of heat by mere conduction, when two bodies of different temperatures are brought into immediate contact; other cases are the production of heat by friction, and by an electric current when overcoming the resistance due to imperfect conductibility, together with all cases where a force, in doing mechanical work, has not to overcome an equal resistance, and therefore produces a perceptible external motion, with more or less velocity, the *vis viva* of which afterwards passes into heat. An instance of the last kind may be seen when a vessel filled with air is suddenly connected with an empty one; a portion of air is then propelled with great velocity into the empty vessel and again comes to rest there. It is well known that in this case just as much heat is present in the whole mass of air after expansion as before, even if differences have arisen in the several parts, and therefore there is no heat permanently converted into work. On the other hand, however, the air cannot again be compressed into its former volume without a simultaneous conversion of work into heat.

The principle according to which the equivalence-values of the uncompensated transformations thus produced are to be determined, is evident from what has gone before, and I will not here enter further into the treatment of particular cases.

In conclusion, we must direct our attention to the function **T**, which hitherto has been left quite undetermined; we shall not be able to determine it entirely without hypothesis, but by means of a very probable hypothesis it will be possible so to do. I refer to an accessory assumption already made in my former memoir, to the effect that *a permanent gas, when it expands at a constant temperature, absorbs only so much heat as is consumed by the external work thereby produced.* This assumption has been verified by the later experiments of Regnault, and in all probability is accurate for all gases to the same degree as Mariotte and Gay-Lussac's law, so that for an *ideal* gas, for which the latter law is perfectly accurate, the above assumption will also be perfectly accurate.

The external work done by a gas during an expansion $dv$, provided it has to overcome a pressure equivalent to its total expansive force $p$, is equal to $pdv$, and the quantity of heat absorbed

thereby is expressed by $\dfrac{dQ}{dv} dv$.    Hence we have the equation

$$\frac{dQ}{dv} = A \cdot p,$$

and by substituting this value of $\dfrac{dQ}{dv}$ in the equation (13), the latter becomes

$$\frac{\dfrac{dT}{dt}}{T} = \frac{\dfrac{dp}{dt}}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

But, according to Mariotte and Gay-Lussac's law,

$$p = \frac{a + t}{v} \cdot \text{const.},$$

where $a$ is the inverse value of the coefficient of expansion of the permanent gas, and nearly $= 273$, if the temperature be given in degrees C. above the freezing-point.    Eliminating $p$ from (15) by means of this equation, we have

$$\frac{dT}{T} = \frac{dt}{a + t}; \quad . \quad . \quad . \quad . \quad . \quad (16)$$

whence, by integration,

$$T = (a + t) \cdot \text{const.} \quad . \quad . \quad . \quad . \quad (17)$$

It is of no importance what value we give to this constant, because by changing it we change all equivalence-values proportionally, so that the equivalences before existing will not be disturbed thereby.    Let us take the simplest value, therefore, which is unity, and we obtain

$$T = a + t. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

According to this, T is nothing more than the temperature counted from $a°$, or about $-273°$ C. below the freezing-point, and, considering the point thus determined as the absolute zero of temperature, T is simply the *absolute temperature*.    For this reason I introduced, at the commencement, the symbol T for the reciprocal value of the function $f(t)$.    By this means all changes which would otherwise have had to be introduced in the form of equations, after the determination of the function, are rendered unnecessary; and now, according as we feel disposed to grant the sufficient probability of the foregoing assumption or not, we may consider T as the absolute temperature, or as a yet undetermined function of the temperature.    I am inclined to believe, however, that the first may be done with hesitation.

XI. *On the Influence of the Earth's Internal Structure on the Length of the Day.* By HENRY HENNESSY, *M.R.I.A.*, *Professor of Natural Philosophy in the Catholic University of Ireland*\*.

THE period of a complete revolution of the earth around its axis of rotation, depends not only on the dimensions of the mass, but also on the distribution of the particles of which it is composed. The variations which in the course of ages may possibly take place in the distribution of these particles will therefore tend to produce some change in the length of the day. The question of the secular variation of the earth's velocity of rotation has been already, to a certain extent, examined by Laplace†; but his investigation has regard solely to the contraction of the dimensions of the globe considered as a slowly cooling solid. If the interior of the earth is in a state of fluidity from heat, or in other words, if it consists of a solid exterior shell filled with a nucleus of matter in a state of fusion, from which state the portion now solid had gradually passed to its present condition, the inquiry assumes a different shape. Hitherto this, as well as every other question connected with the general structure and rotation of the earth, has been treated on the assumption that the portions composing the fluid underwent no change in their positions on entering into the solid state. This assumption formed not only the basis of the inquiries of Laplace, but even of more recent investigators, who professed to consider the purely physical conditions of problems relating to the structure of our planet in a far more complete manner. The change of state of the matter composing the interior of the earth in passing from fluidity to solidity seems to have been thus entirely overlooked, although a little reflection might have suggested that such a change would possibly influence its structure to a very considerable extent.

In the first part of my " Researches in Terrestrial Physics‡," the necessity of attending to this circumstance was distinctly pointed out, and the superfluous nature of the contrary assumption was formally declared. Experiments were quoted in the second part confirmatory of my views on this point, which I believe have never since been called in question. When we reflect on the remarkable results of the researches of Professor Bischof, M. Ch. Sainte-Claire Deville, and M. Delesse, upon the contraction of the principal materials of the solidified crust of the earth in passing successively from the fluid state through

---

* Communicated by the Author.
† *Mécanique Céleste*, livre xi.
‡ Philosophical Transactions, 1851, part 2, p. 495.

the vitreous condition up to that of complete crystalline struc-
ture, it is at once apparent that the influence of changes of the
state of molecular aggregation of such masses must exercise an
important influence on the general structure of the earth, as well
as the slow cubical contraction of the entire mass during the
process of its refrigeration.

In my second memoir, I showed the way in which the former
species of contraction would affect the internal structure of the
earth supposed to consist of a solid shell and included nucleus
of fluid.   The process of solidification of the fluid, commencing
from the centre and ending at the surface, according to the
views of Poisson, was proved to be impossible ; and the only way
in which the process could take place, was shown to be by suc-
cessive additions of matter from the surface of the nucleus to the
inner surface of the solid shell.   Each outer stratum of the
nucleus would thus successively become a stratum of the shell.
But the density of any stratum of equal pressure in the fluid
depends on the pressure of all the strata by which it is enve-
loped, and therefore the removal of these strata in regular suc-
cession, gradually tending to decrease the pressure, must decrease
the density of the stratum in question.   This action, operating
on all the strata of the nucleus, will manifestly give it a tendency
to enlarge its volume so as to fill up the space left by the con-
traction of its exterior strata.   This expansion of the nucleus
will evidently be accompanied by a diminution of its mean den-
sity, and has been shown to be also attended with a change in
the law of density in going from its centre to its surface.   The
density will vary less rapidly as the solidification of the mass
advances, and the fluid will tend to become more homogeneous.
The mass of the shell is at the same time continually augmented
in a corresponding degree by the successive additions it thus
receives at its inner surface, so that the aggregate effect of the
process of solidification is the removal of matter from the centre
towards the surface of the earth.

An admirable example of the effect of internal forces on the
solidification of a cooling mass of fused igneous rock, which will
serve in some measure to illustrate my remarks, has been de-
scribed by Mr. Darwin in his ' Naturalists' Voyage*;' it presents
a case where the forces tending to expand the liquid enclosed in
the first solidified envelope are extremely energetic, and the order
of the phænomena is thus very clearly exhibited.   He noticed in
several places in the Island of Ascension volcanic bombs, which
have been shot through the air while in a fluid state, and which,
spinning around their centres as they passed through their course,
have usually assumed a rounded shape.   He then remarks, that

* Page 493.

not only the external form, but in several cases their internal structure, shows that these bodies have revolved in their aërial course. One of these bombs when broken through presented the following appearances :—1. The central part was highly cellular, the cells decreasing in size from the centre towards the exterior. 2. These cells terminated at a shell-like case of compact stone about a third of an inch in thickness. 3. Outside this case was a crust of finely cellular lava. Mr. Darwin explains these phænomena very rationally, by saying that the outermost crust cooled rapidly into the state in which it came under his observation ; then the enclosed fluid was pressed against the shell so formed by centrifugal force arising from the rapid rotation of the mass, thus producing the compact stony casing ; lastly, the action of the same force relieving the pressure of the fluid at its central portions, the expansive tendency of the included elastic vapours would ultimately produce the coarse cellular structure at the centre. It might be added, that the passage of the fluid into the solid state, when forming the compact case of stone, being necessarily accompanied by contraction, would allow some space for the operation of the expansion of the remaining fluid.

If the removal of matter from the interior towards the exterior portions of the earth took place equally along every radius drawn from the centre to the surface, all of the earth's moments of inertia would be augmented, and the length of the day, so far as it could be affected by this cause, would of course be increased. The contraction of the solidifying surface of the nucleus upon the inner surface of the shell, in passing from the fluid to the solid state, taking place from within outwards, its effect could not be that suggested by M. Delesse*, in terminating his valuable remarks on the crystalline contraction of rocks, namely a diminution of the earth's radius, and a consequent increase in the velocity of rotation, but precisely the reverse.

On the old assumption of mathematical investigators, that the particles composing the earth retained the same positions on passing from the fluid to the solid state, the oblateness of the strata of equal density of the solidified mass would correspond to the surfaces of equal pressure in the fluid, and would be less and less oblate in going from the outer to the inner surface of the shell. By discarding this assumption, and admitting the influence of the physical changes that take place in the passage of the fluid matter of the nucleus to the state of solidity, I have shown that the process of solidification already briefly described would tend to augment the oblateness of the strata of the shell in going from its outer to its inner surface. From this conclu-

* *Comptes Rendus,* vol. xxv. p. 545.

sion another is immediately deduced, which is of fundamental importance in connexion with the question now under consideration. It follows that the shell and nucleus must rotate together very nearly as if one mass, for otherwise, according to a result obtained by Mr. Hopkins*, the annual precession of the equinoxes would greatly differ from that which is observed. Hence it follows that great friction and pressure must exist at the surface of contact of the nucleus and shell, and probably also even between the particles of the fluid. Such a state of things should indeed be expected *à priori* from the highly crystalline structure, and probably unequal surface of the shell, as well as the viscous nature of the fused materials of the earth, so far as we can judge from those coming under our notice†. The existence of great pressure has been already indicated in connexion with the gradual tendency of the nucleus to expand its volume.

While the strata of equal density in the shell would thus increase in oblateness from the outer to the inner surface, the strata of equal pressure in the nucleus would always follow the opposite law, of decreasing in oblateness from the surface to the centre. I have shown that this result, combined with that which has been just mentioned, would lead to another directly connected with the question of the earth's rotation, namely that the difference between the greatest and least moments of inertia of the earth continually tends to increase during the process of solidification, and consequently that the stability of the earth's axis of rotation, so far from being disturbed by that process, is increased during its successive stages. This point was further developed in a letter to Sir John Lubbock‡, replying to some communications with which he had favoured me with reference to a short paper he had inserted in the Journal of the Geological Society of London, wherein he had endeavoured to show the possibility of a change in the position of the earth's axis of rotation from the effect of physical changes in its structure.

The increase of the difference between the greatest and least moments of inertia of the earth would be chiefly due to an absolute increase of the former. As this corresponds to the present axis of rotation, it follows, that not only would the stability of the earth's axis of rotation be more completely assured, but also that its velocity of rotation would be diminished. This conclusion, combined with that already arrived at from a general consideration of the influence of the process of solidification, seems to establish the existence of a tendency to increase the length of the day. At the same time the slow cubical contrac-

   * Phil. Trans. 1840, p. 207 ; see also Phil. Trans. 1851, p. 546.
   † See Berzelius in Poggendorff's *Annalen*, vol. xliii.
   ‡ Proceedings of the Royal Society, February 1852.

tion of the entire spheroid, from the gradual cooling of all its particles, would lessen its dimensions, and thus tend to accelerate the velocity of rotation so as to diminish the length of the day. The energy of both of these opposing tendencies depends upon a common cause, the rate of cooling of the entire earth. This has been demonstrated by Fourier and other illustrious mathematicians, on the most favourable suppositions to rapidity of refrigeration, to be so extremely slow, that if only one of the counteracting influences here adduced existed without the other, we could scarcely expect to discover its action on the rotation of the earth until after a long period of exact observations. When the fact of the simultaneous existence and opposition of these influences is remembered, it should not excite surprise that astronomical observations should have hitherto never disclosed any variation in the length of the day, and ages may possibly elapse before any such variation will be discovered.

XII. *On Heat as the Equivalent of Work.*
*By* W. J. Macquorn Rankine, *C.E., F.R.SS.L. & E.*

*To the Editors of the Philosophical Magazine and Journal.*

Gentlemen,

IN the Number of your Magazine for July (at p. 75) there appears an abstract of a paper by M. Hoppe, first published in Poggendorff's *Annalen,* vol. xcvii. p. 30, commencing, "Hoppe has contributed a memoir upon this most interesting and important subject, which places the analytical theory in a remarkably clear, simple, and general point of view, so far at least as it relates to permanent gases." This observation naturally leads the reader to infer, that the theory previous to the publication of M. Hoppe's paper was deficient in clearness, simplicity, and generality; and on first reading it in Silliman's Journal, I contemplated entering into a detailed discussion of M. Hoppe's paper; but this intention I have since abandoned, on finding that that discussion has been already made by Professor Clausius (Poggendorff's *Annalen,* vol. xcvii. p. 173) in a short paper, which appears to me to be well worthy of publication in the English language. I shall therefore confine my remarks to stating, that the whole theory of heat as the equivalent of work performed by the expansion of elastic substances, whether gaseous, liquid, or solid, is summed up in this one equation:—

$$\mathrm{J}dq = \mathrm{J}kd\tau + \tau\left(d\tau\frac{d}{d\tau} + dv\cdot\frac{d}{dv}\right)\int\frac{dp}{d\tau}dv; \quad . \quad . \quad \text{(A)}$$

where J is Joule's equivalent; $dq$ the quantity of heat received by the substance during the increase of absolute temperature $d\tau$

and of volume *dv*; *k* the real specific heat of the substance; *p* the pressure under which it expands, so that *pdv* is the external work performed by the expansion. The various expressions for this equation which have appeared in the writings of Messrs. Clausius and Thomson, and in my own, merely differ in form, and are all substantially equivalent to each other. It affords the solution of every conceivable question where the mutual relations of heat and of work by cubic expansion are concerned, and has been abundantly and rigorously verified by experiment; and I think I am justified in maintaining it to be clear and simple as well as general.

I trust it will be understood, that, in making this statement, I have no wish to detract from the merits of M. Hoppe, my sole object being to defend the existing theory against the prejudicial inference which might be drawn from the opening remark of *the English abstract* of M. Hoppe's paper—a remark which does not appear in the original German.

I have the honour to be, Gentlemen,

Your most obedient Servant,

Glasgow, July 2, 1856.                    W. J. Macquorn Rankine.

P.S. I may take this opportunity of giving increased publicity to a peculiar transformation of the equation A, which is useful in certain special investigations. The paper in which it was first given was read to the Royal Society of Edinburgh in February 1855, but has not yet been published.

$$\mathrm{J}dq = \left(\mathrm{J}k + \frac{p_0 v_0}{\tau_0}\right)d\tau - \tau\left(d\tau\frac{d}{d\tau} + dp\frac{d}{dp}\right)\int\frac{dv}{d\tau}\,dp. \quad . \quad (\mathrm{B})$$

$p_0$, $v_0$, $\tau_0$ denote the pressure, volume, and absolute temperature of the substance in the ideal state of perfect gas.—W. J. M. R.

---

XIII. *On the Demonstration of* Fresnel's *Formulas for Reflected and Refracted Light; and their Applications.*—Part II. *By the* Rev. Baden Powell, *M.A., F.R.S. &c., Savilian Professor of Geometry in the University of Oxford*[*].

IN a former paper (see Phil. Mag. &c. July 1856) I have placed in a connected point of view the several principles and deductions leading to the well-known formulas of Fresnel, as well as to certain modifications of them, for the amplitudes of the vibrations of the incident, reflected, and refracted rays, whether polarized parallel or perpendicular to the plane of incidence. I have also remarked on the question which has so long

divided opinions, whether the *vibrations* are *parallel* or *perpendicular* to the plane of *polarization,* and on the decisive evidence lately obtained in favour of the latter hypothesis. Some other questions relative to the same subject still demand examination, to which I propose now to refer.

2. For this purpose it will be necessary briefly to premise a *recapitulation* of the primary principles on which the several investigations proceed, and which are fully discussed in my former paper. These are,—

I. The principle of *vis viva* ; (1) that the square of the velocity multiplied by the vibrating mass is the true measure of force ; (2) that the *vis viva* of the *incident* vibrations is equal to the sum of the *vires vivæ* of the *reflected* and *refracted* vibrations. Or, $m$ and $m_i$ being the simultaneously vibrating masses of æther without and within the medium, $h$, $h'$, $h_i$ respectively the amplitudes (which are the measures of the velocities) of the incident, reflected, and refracted vibrations ;—then the law of *vis viva* is expressed by the equation

$$m(h^2 - h'^2) = m_i h_i^2.$$

II. The law of equivalent vibrations, which on Maccullagh's view is expressed by

($\alpha$) . . . $h + h' = h_i$ for vibrations *perpendicular* to the plane of incidence,

and ($i$ and $r$ being the angles of incidence and refraction)

($\beta$) . . . $h + h' = h_i \dfrac{\cos r}{\cos i}$ for vibrations *parallel* to the plane of incidence.

III. On the principle adopted by Fresnel in the second case ($\beta$), the same law would be expressed by

($\alpha$) . . . $h - h' = h_i$ for vibrations *perpendicular* to the plane of incidence,

($\beta$) . . . $h - h' = h_i \dfrac{\cos r}{\cos i}$ for vibrations *parallel* to the plane of incidence.

IV. Maccullagh's hypothesis of equal density giving

$$\frac{m}{m_i} = \frac{\sin 2i}{\sin 2r}.$$

V. Fresnel's hypothesis of increased density giving

$$\frac{m}{m_i} = \frac{\sin r \cos i}{\sin i \cos r}.$$

VI. Maccullagh's hypothesis of vibrations parallel to the plane of polarization.

VII. Fresnel's hypothesis of vibrations perpendicular to the plane of polarization.

3. By different combinations of these principles, different modifications of the formulas result. Thus we have the hypotheses—

(A) combining Nos. I. II. IV. VI., whence are obtained the formulas

$$h' = \frac{\sin (i-r)}{\sin (i+r)} \ldots . \quad h_{\prime} = \frac{\sin 2i}{\sin (i+r)} \ldots . \text{ vibrations } parallel,$$

$$k' = \frac{\tan (i-r)}{\pm \tan (i+r)} - \ldots . k_{\prime} = \left(1 + \frac{\tan (i-r)}{\pm \tan (i+r)}\right) \ldots perpendicular,$$

which are Maccullagh's formulas ; the double sign indicating the change at the polarizing angle.

(B) Combining Nos. I. II. V. VII., whence are obtained,

$$h' = \frac{-\sin (i-r)}{\sin (i+r)} \quad h_{\prime} = \frac{2 \sin r \cos i}{\sin (i+r)} \ldots . \text{ perpendicular,}$$

$$k' = \frac{-\tan (i-r)}{\pm \tan (i+r)} \quad k_{\prime} = \left(1 - \frac{\tan (i-r)}{\pm \tan (i+r)}\right) \frac{\cos i}{\cos r} \ldots . \text{ parallel.}$$

(C) Combining Nos. I. III. V. VII., whence are obtained,

$$h' = \frac{\sin (i-r)}{\sin (i+r)} \quad h_{\prime} = \frac{2 \sin r \cos i}{\sin (i+r)} \ldots . \text{ perpendicular,}$$

$$k' = \frac{\tan (i-r)}{\pm \tan (i+r)} \quad k_{\prime} = \left(1 - \frac{\tan (i+r)}{\pm \tan (i+r)}\right) \frac{\cos i}{\cos r} \ldots . \text{ parallel.}$$

4. Each of these two last sets differs from Fresnel's in the *signs*. Fresnel's *original* formulas can only be produced from assuming hypothesis (B) for *h,* and (C) for *k*; or we have,—

(D) combining Nos. I. II$\alpha$. III$\beta$. V. and VII., whence are obtained,

$$h' = \frac{-\sin (i-r)}{\sin (i+r)} \quad h_{\prime} = \frac{2 \sin r \cos i}{\sin (i+r)} ,$$

$$k' = \frac{\tan (i-r)}{\pm \tan (i+r)} \quad k_{\prime} = \left(1 - \frac{\tan (i-r)}{\pm \tan (i+r)}\right) \frac{\cos i}{\cos r} ,$$

which are Fresnel's original formulas.

5. With regard to the law of equivalent vibrations, it may indeed be remarked that Prof. Maccullagh in stating it, with a view to his ulterior researches on crystalline reflexion, rather *assumes* than *demonstrates* the main principle, and thus the modified form of that law (No. III.) may possibly be as open to consideration as the original form.   But as *neither* form exclusively will produce Fresnel's *original* formulas, it becomes of more importance to look to some other principle which might

account for Fresnel's adoption of No. III. in one case ($\beta$) and not in the other ($\alpha$). That is, for vibrations *parallel* to the plane of incidence this form is avowedly adopted by him, on the ground that for that case the sign of the reflected vibration will be opposite to that of the incident at small incidences. But in the case of vibrations *perpendicular* to the plane of incidence, he not only makes no assumption of this form (III$\alpha$.), but his result being deduced directly from the analogy of impact—if this is to be analysed up to the deduction of the same equation from the principle of the *vis viva*—it is evident (as shown in my former paper (37)) that that analysis involves necessarily the assumption of the law of equivalence No. II. Fresnel himself, however, did not so deduce the equation, but appears to have simply assumed it on the analogy of impact of elastic bodies.

6. The assumption thus made by Fresnel (which he admits to be somewhat empirical) in the one case, while no such assumption is made in the other, may possibly be accounted for on the consideration, that, in the case of vibrations parallel to the plane of incidence alone, we can have any direct application of the parallelogram of forces, from the construction of which he may possibly have been led to this inference. In the other case, where the vibrations are all perpendicular to the plane of incidence, and parallel to the surface and to each other, no such construction can apply; nor does he seem to have extended the idea of mechanical equivalence by analogy to this case, as Maccullagh has done.

7. The crucial experiment of Professor Stokes obliges us to set aside the supposition No. VI. (see former paper (62)), and consequently (as the analysis shows) the supposition No. IV., and thus the whole hypothesis (A) or Maccullagh's formulas. Our choice then lies between the other hypotheses, or some new combination. And it remains to decide whether the *original form* of Fresnel's formulas is of necessity required by any experimental results, or whether the forms deduced on either of the hypotheses (B) or (C) will be equally applicable; in other words, whether *the difference in the signs* is of importance.

Now in fact two well-known cases of experimental results are adducible which *appear* to have a direct bearing on this question, in which light we will proceed to examine them.

### *Change in plane of Polarization, by Reflexion.*

8. It is a result long ago ascertained by the researches of Fresnel, Arago, and Brewster (and, as far as the principal point is concerned, easily capable of verification), that if a ray previously polarized in a plane inclined at a given angle to the plane of incidence fall on a reflecting surface, then, after reflexion, *in*

*general its plane of polarization is changed*; at incidences *less* than
that of complete polarization, the new plane of polarization *deviates* on the side of the plane of incidence *opposite* to that of the
original plane of polarization; at the incidence of complete polarization it *coincides* with the plane of incidence; at incidences
*greater* than that of complete polarization it *deviates* on the *same
side* as the original plane.

9. Now these are precisely the changes indicated by a very
simple deduction of theory, derived from the *original* formulas of
Fresnel. The deduction is well known, but it will be desirable
to exhibit its nature explicitly as follows:—

Let the original plane of polarization (P) be inclined to that
of incidence (I) by an angle $(\alpha)$, then after reflexion the parts of
the amplitude resolved parallel and perpendicular to (I), viz.—

$$k' \sin \alpha = \frac{\tan (i-r)}{\pm \tan (i+r)} \sin \alpha \text{ and } h' \cos \alpha = \frac{-\sin (i-r)}{\sin (i+r)} \cos \alpha,$$

will by composition give a resultant ray polarized in a plane (Q)
inclined to (I) by an angle $(\beta)$, where

$$\tan \beta = \left(\frac{\pm k'}{-h'}\right) \tan \alpha.$$

Hence at first, the tangents having opposite signs, the arcs $\alpha$
and $\beta$ lie in *adjacent* quadrants; at $i+r=90° \beta=0$; and for
incidences greater, the tangents having the same sign, $\alpha$ and $\beta$
lie in the *same* quadrant, which exactly expresses the experimental
results.

10. It should be remarked that this is the reasoning adopted
by Mr. Airy (§ 132), and that the undeniable conclusion follows
simply by virtue of the *symbols*, without the introduction of any
extraneous construction or subsidiary consideration whatever.

11. Now Dr. Lloyd (in his 'Lectures on the Wave Theory,'
part 2, p. 35), after stating the facts, gives the same deduction
from theory, using *Maccullagh's* formulas; but with due caution
makes the inference only so far as to show the *existence* of a deviation to the same *amount, without expressing in which direction.* But
according to *these* formulas, $\tan \beta$ is *at first positive*, and changes
to *negative* at the polarizing incidence. Hence as to the *direction*
of the deviation, *if the former reasoning be correct,* then in this
case, *on the same grounds,* the symbols would indicate that the
deviation at incidences *less* than that of polarization must be on
the *same* side as the original plane, and *after* that incidence, on
the *opposite* side; which is the *reverse* of the *former* conclusion,
and of *the fact.*

12. This, however, would appear to be only an additional
reason to that already assigned for the rejection of Maccullagh's

theory. But it is easily seen that precisely the same remarks apply if, instead of Maccullagh's formulas, we adopt either of the new forms of Fresnel's, viz.—

$$\text{(B) which gives } \left(\frac{\mp k'}{-h'}\right),$$

or

$$\text{(C) which gives } \left(\frac{\pm k'}{+h}\right).$$

The result of experiment would therefore *seem* equally decisive against the hypothesis (A), and against (B) and (C) applied to *both* $h'$ and $k'$, and would leave us to the sole adoption of Fresnel's original formulas (D). But before coming to this conclusion, we must advert to another experimental case in which similar considerations are involved.

### *Dr. Lloyd's Interference Experiment.*

13. In an experiment long ago devised by Dr. Lloyd, a *direct* ray interferes with one *reflected* at a *very oblique* incidence from a plate of glass; giving a set of stripes *resembling* generally one half of those formed in the experiment of Fresnel when *two* streams of *direct* light interfere (Mem. Roy. Irish Acad. vol. xvii. 1834).

In this experiment, it is first to be remarked that the first dark band is *intensely black*, proving the *equal* intensity of the interfering rays; in agreement with theory, which at the extreme incidence gives, on either hypothesis, reflected light of equal intensity with the incident (see former paper (§ 58)).

14. The stripes, however, present this peculiarity : *on ordinary suppositions*, since the rays at the extreme limit must be in *accordance*, there would first be *half a bright band* (reckoning from the edge) followed by a whole dark, then a whole bright band, and so on ; whereas, in fact, the first bright band is observed to be of the *entire breadth* of an interval, as if the system commenced from a point of *discordance* at the extreme limit, or as if the whole were *shifted* from the edge through a distance equal to *half the interval* of two bands. This was found to be equally the case whether the light was previously polarized in a plane *parallel* or *perpendicular* to the plane of incidence.  In other words, the fact indicates a difference in phase of 180° between the incident and the reflected ray at the limit, and this equally in each case of the direction of polarization.

15. Now in Fresnel's original formulas for vibrations perpendicular to the plane of incidence, a difference of *sign*, indicative of this difference of *phase*, occurs at all incidences in the formulas for $h'$, and for vibrations *parallel* to the plane of incidence at

all incidences greater than that of complete polarization in the formulas for $k'$; that is, at very *oblique* incidences for rays polarized in one plane equally with those in the other; in exact agreement with the observed fact.

16. This is the argument expressly adduced by Dr. Lloyd, derived *directly*, without any subsidiary considerations, from the indications of the symbols; and it has, I believe, been generally received as perfectly clear and conclusive. It should be remembered that Dr. Lloyd's paper was communicated and published in 1834, and refers to Fresnel's formulas *in the form in which he originally gave them*, and before any modifications of the theory had been contemplated. Prof. Maccullagh's new views and formulas were first communicated to the Royal Irish Academy in 1837, and published in its Memoirs in 1838. Thus Dr. Lloyd's reasoning is of necessity wholly independent of any speculations on these newer principles, which, if applied to Fresnel's theory, give the formulas with different *signs*.

17. Now so far as the mere symbols are concerned, we find in Fresnel's formulas (B) this difference of sign, at great incidences, occurring only for $h'$; and in the formulas (C) (as is the case also in Maccullagh's formulas) only for $k'$; while in (B) for $k'$, and in (C) for $h'$, no such difference occurs.

This result would therefore *seem* decisive in favour of Fresnel's *original* formulas (D), to the *exclusion* not only of Maccullagh's, but of Fresnel's, in the entire forms (B) and (C).

18. The reasoning both in this case and the former (9), (10), is indeed of a nature apparently so obvious, that I should not have thought it necessary to state it in detail, were it not that some considerations *have been suggested* which seem to set it aside, or at any rate to require a closer review of its meaning;—and which may be stated as follows:—

19. For vibrations *parallel* to the plane of incidence, if we make a construction of the course of the rays, then, at an incidence very near the perpendicular, the vibrations $k$ and $k'$ respectively perpendicular to the incident and reflected rays will lie very nearly in one line; and if we suppose them in the *same* direction, then in passing to an incidence extremely oblique, they will come into directions *opposite* to each other, owing merely to the *position* the rays have now assumed.

Hence it is inferred, whatever relative directions of the vibrations we express by the signs $+$ or $-$ at $i=0$ will be reversed when we pass to $i=90°$. And thus, for example, in the formula (B$k'$), if at $i=0°$ we suppose $-k'$ and $+k$ to express *opposite* directions, then at $i=90°$ the *same* signs would express *accordant* directions. But in fact at $i=90°$ we have $+k'$ and $+k$, which on this principle therefore now express opposing directions.

20. In the case of Fresnel's and Arago's result (see above, (9)) of the change in the plane of polarization after reflexion, it should be remarked that the reasoning turns wholly upon the *signs* of the resulting equation for the *tangents* of the two related arcs; and these are derived from the relative *signs* of the quantities which enter into the numerator and denominator *simply as algebraic quantities, and without any reference whatever to the interpretation* of those signs as expressive of *difference of phase, or any other physical conditions.* The resulting relation is a purely trigonometrical one, and the direction in which the arcs are to be relatively measured would be just the same whatever might be the physical theory to which they were to apply. *This* case therefore can in no way be affected by any theory of the change of direction due to the position of the vibrations. Here, then, Fresnel's original formulas exclusively apply.

21. The case then stands thus :—From the experiment of Prof. Stokes, Maccullagh's formulas are set aside. From the experiments of Lloyd and Arago, so far as relates to vibrations *perpendicular* to the plane of incidence, Fresnel's formula ($Ch'$) is set aside, since it does not apply directly ; and for such vibrations the subsidiary theory as to the signs is inapplicable ; while Fresnel's formula ($Bh'$) fully agrees with experiment without any subsidiary explanation. *This formula then must necessarily be adopted exclusively.*

22. It is only, then, in the case of vibrations *parallel* to the plane of incidence that the question remains between Fresnel's formula ($Ck'$), which applies *directly* to the experimental results, but rests on an hypothesis (No. III.) of equivalent vibrations open to question,—and his formula ($Bk'$), which rests on Maccullagh's law of equivalent vibrations (No. II.), but does not apply to the experimental facts without a subsidiary interpretation as to the *signs.*

23. Now any subsidiary construction, if necessary for the right interpretation of a formula, only shows that *that formula is symbolically incomplete,* and does not include the expression of the *whole* case it is designed to represent. This, then, would be an additional reason for rejecting that formula and adopting the former in preference.

24. But this construction (19) is in itself not free from question and difficulty. For let us only consider the case at a point *upon the surface* : here we have an incident *rectilinear* vibration in a determinate direction ; but at its point of incidence it gives rise to a nascent *circular* wave, in which it is impossible to say that the vibration is more in one direction than another ; it has no rectilinear direction till we come to take the common tangent (as in the ordinary explanation of reflexion) to two successive waves ;

and there is nothing to determine the direction of vibration at the points coinciding with this tangent. We cannot therefore make any assumption as to its constancy at different incidences; there is nothing to show that it may not change from one incidence to another; and we know that it does undergo such a change at the incidence of complete polarization.

25. On the other hand, the formula ($C k'$) applies *directly* to the phænomena without the aid of any subsidiary construction whatever, *unless indeed the whole principle of the reasoning hitherto adopted be contested.*

But if we may still accept the reasoning of Fresnel and Arago, of Airy and Lloyd, as valid, then it follows that *both* Fresnel's *original* formulas, and they *alone*, will apply directly to all the experimental results without any extraneous considerations. But one of them ($k'$) rests on an assumption as to the law of equivalent vibrations *different* from the other ($h'$) ; and the question remains, whether that difference of assumption can be justified, or whether any other view of the theoretical principles can be found to lead to the same results.

I have thus stated in full detail the difficulties of this important case; and will only add, that I shall look with great interest to any attempts at removing them, which I hope this representation may be the means of eliciting from those mathematicians who have attended to the subject.

---

XIV. *Letter on* Professor Galbraith's *Construction for the Range of Projectiles.    By* J. J. SYLVESTER, *Professor of Mathematics at the Royal Military Academy.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

PROFESSOR GALBRAITH'S geometrical construction for finding the elevations of a projectile corresponding to any given velocity and given range in a plane, horizontal or sloping, is truly elegant, and, if new, constitutes a real acquisition to the subject. It might be worth while for its accomplished author to see if some analogous construction can be found extending to the more general case where the field is a portion of a circle. I need hardly add that the *isoscelism* referred to is, except for some extreme suppositions (impossible to occur in practice), absolutely independent of the form of the field.

As well-constructed names are, in fact, condensed lessons, lending an aid to the memory and imagination, of which modern mathematicians are only beginning to appreciate the importance, I suggest the following designations.

The point of *projection* and point of *impact* speak for themselves; the point vertically over the point of impact in the direction of projection may be called the point of *aim*. The line joining the point of aim and the point of impact is the *drop* or *fall*; the line joining the point of projection and the point of impact may be called the *excursion*; and that joining the point of projection and the point of aim, the *length of aim*.

A vertical section of the ground (plane or curved) through the axis of the gun may be called the *field*. We may then say, that, for the maximum range, the fall is always equal to the excursion, whatever the form of the field; and that in general the locus of the point of aim, for a rectilinear field when the point of the projection and the velocity are given, is a circle to which, in the case of the angle of best elevation, the line of fall is of course a tangent. It would not be surprising if a good deal of elegant geometry (like ivy twining round an old wall) should hereafter associate itself with Mr. Galbraith's " circle of aim : " *à propos* of projectiles, it is not unworthy of observation, that the velocities at any two points P and Q of the parabolic path are as the lines PT, QT which the tangents at P and Q mutually cut off from one another, a remark which of course is easily seen to extend itself to the case of an elliptic orbit with the force in the centre.

<div style="text-align:center">Ever, Gentlemen,<br>Your faithful friend and reader,</div>

Woolwich Common,                            J. J. Sylvester.
July 3, 1856.

P.S. The value of Mr. Galbraith's method consists simply in the *act of conception* of the locus of the point of aim ; it was scarcely worth while (at this time of day) to append a synthetical proof of so simple a proposition, which may be got at immediately by calling the length of aim $\rho$, its inclination to the vertical, $\theta$, and that of the field to the vertical, $i$; when by similar triangles (if H denote the quantity $\dfrac{2v^2}{g}$, and $\eta$ the vertical distance of the point of projection from the field) we obtain the equation

$$\frac{\dfrac{\rho^2}{H} - \eta}{\rho} = \frac{\sin (i - \theta)}{\sin i},$$

or

$$\rho^2 - H \frac{\sin (i - \theta)}{\sin i} \rho - H\eta = 0 ;$$

which obviously corresponds to the circle of Professor Galbraith.

I imagine this circle has been long known for the case of the point of projection being in the field, but it may have escaped notice for the more general case. The equality between the *fall* and the *excursion* for the angle of maximum range subsists, not merely for a rectilinear or curved section, but for the ground itself (whatever its form of surface) when the gun is supposed to admit of being laid to any angle, as well as at any elevation.

XV. *On the Variations in the Chemical Composition of the Thames Water, during the year between May* 1855 *and May* 1856. *By* HENRY M. WITT, *F.C.S., Assistant Chemist to the Government School of Science Applied to Mining and the Arts*[*].

SO numerous are the chemical analyses of the water of the Thames which have been published during the last few years, and by chemists of such eminence have they been made, that it would appear altogether superfluous to make further additions to their number; but one most important fact has scarcely attracted sufficient attention, that any single analysis, however carefully and accurately executed, represents only the composition of that particular sample of water under examination, and not the general or average composition; in fact the quantities of the several impurities of the water of the Thames differ, as might be expected, most essentially *at the various points in its course*, and *at the several seasons of the year*.

To the variations in composition at different points of the river's course attention has been drawn long since, especially in the Report by Professors Graham, Miller, and Hofmann, to the Secretary of State for the Home Department, made in 1851[†], by whom it was recommended that the supply of London should in future be taken from some point above the tidal influence, further removed from the contamination of the London sewage.

But great discrepancies have often occurred between the analyses of the water at any particular point made by different experimenters, due doubtless to the changes which take place at different seasons of the year; and it became evident that a correct opinion of the composition of the water, at any special point of the river's course, could only be formed by making a *large number of analyses* extending over a considerable *space of time*, and from them deducing the *mean composition*.

This desideratum was partially supplied by Dr. Robert Dundas Thomson, who, in the Quarterly Journal of the Chemical Society for July 1855[‡], published the results of a series of analyses made

between the 1st of September 1854 and the beginning of the year 1855; but it is obvious that this includes only a quarter of a year, and, moreover, only that particular quarter following the drought and heat of summer and autumn, the effect of the spring rains being altogether omitted.

The analyses detailed below extended *over the whole year*, viz. from May 1855 to May 1856; so that from them the maximum, minimum, and *mean* composition may be deduced for the whole year.

Two series were undertaken, the one at Kingston, the present source of supply of the Chelsea and Lambeth Waterworks' Companies, the other at Chelsea, with the view of comparing the *mean* composition at these two points : the analyses of the former series were repeated about once a month (and, indeed, during a certain period when great fluctuations took place, once a week), and those of the latter four times in the year, viz. in June, October, and December 1855, and May 1856.

At the commencement of the inquiry a complete analysis was made at each point, the results of which are given in detail; but subsequently only the most important constituents were determined, and the analyses are given as the direct results of experiments (without arranging the acids and bases together), to facilitate present and future comparison.

To commence with the composition of the water at Kingston : the following are the results of the original complete analysis.

This sample was collected at the Chelsea Waterworks Company's new works at Kingston (in common with all the other samples from this point, by my friend Mr. James Simpson, jun.), on the 26th of May, 1855. It had a temperature at the time of 62° F. (16°·6 C.), that of the air being 63° F (17°·22 C.).

The following are the analytical results :—

TABLE *a*.—Direct results of Experiments calculated in 1000 grains of the Water.

| Experiment. | Lime. | Magnesia. | Chlorine. | Sulphuric acid. | Alkaline chlorides. | Potassium. | Sodium. | Suspended clay, sand, &c. |
|---|---|---|---|---|---|---|---|---|
| 1 | ·10424 | ·00707 | ·0144 | ·03722 | ·0477 | trace | ·0187 | ·0529 |
| 2 | ·1026 | ·00616 | ...... | ·03852 | ·03852 | ...... | ·0235 | |
| Mean... | ·10342 | ·0066 | ·0144 | ·03787 | ·03787 | ...... | ·0211 | ·0529 |

| | Inorganic residue. | Organic matter. | Total residue. |
|---|---|---|---|
| Mean of three experiments.. | ·3260 | ·0233 | ·3493 |

TABLE *b.*—Mean of the preceding calculated in grains in the gallon.

| | |
|---|---|
| Inorganic residue . . . . | 22·820 |
| Organic matter . . . . | 1·631 |
| Total residue . . . . | 24·451 |

| | grs. |
|---|---|
| Lime . . . . . . . . | 7·240 |
| Magnesia . . . . . . | 0·462 |
| Chlorine . . . . . . | 1·008 |
| Sulphuric acid . . . . . | 2·651 |
| Alkaline chlorides . . . . | 3·7669 |
| Potassium . . . . . . | trace |
| Sodium . . . . . . . | 1·480 |
| Suspended clay and sand . | 3·603 |
| Ammonia (as carbonate) . . | ·0034 |

TABLE *c.*—Saline Constituents in the gallon.

| | grs. |
|---|---|
| Sulphate of lime . . . . | 4·506 |
| Carbonate of lime . . . . | 9·616 |
| Carbonate of magnesia . . . | 0·970 |
| Chloride of sodium . . . | 1·661 |
| Chloride of potassium . . . | trace |
| Carbonate of soda . . . . | 1·950 |
| Organic matter . . . . . | 1·631 |
| Suspended clay . . . . . | 3·603 |
| Carbonate of ammonia . . . | 0·0034 |
| Total . . . . . . . | 23·9404 |

Total residue obtained by experiment   24·451

The hardness by Dr. Clarke's soap-test was—

| | |
|---|---|
| Before boiling . . . | 11° |
| After boiling . . . | 6° |
| Loss by boiling . . | 5° |

The following Table shows the comparison of this analysis

with the subsequent determinations of the more important constituents, with the dates when the samples were collected :—

TABLE I.—Analysis of Thames Water at Kingston, 1855–56.

(Direct experimental results.)

| Date of collection. | May 26, 1855. | Sept. 8, 1855. | Oct. 8, 1855. | Nov. 3, 1855. | Dec. 29, 1855. |
|---|---|---|---|---|---|
| Temperature of the water... | 62° F. | 60° F. | 57° F. | 42° F. | 42° F. |
| | Grains in the gallon. | | | | |
| Total residue (including suspended matter) ............ | 24·45 | 18·37 | 23·949 | 22·750 | 24·35 |
| Suspended matter (clay and sand) ........................ | 3·603 | ...... | ...... | 3·107 | 3·82 |
| Organic matter (in solution) | 1·631 | 1·025 | 1·430 | 1·430 | 0·94 |
| Total dissolved salts ......... | 19·217 | ...... | ...... | 18·213 | 19·59 |
| Chlorine ........................ | 1·008 | 1·116 | 1·054 | ...... | 1·287 |
| Sulphuric acid ............... | 2·651 | ...... | 3·635 | 2·065 | 3·87 |
| Lime ............................ | 7·240 | ...... | 6·487 | 6·795 | 7·73 |
| Magnesia ...................... | 0·462 | ...... | 1·213 | | |
| Hardness before boiling ... | 11° | 12·28° | 7° | 11·0° | 11·5° |
| Hardness after boiling ...... | 6 | ...... | ...... | ...... | 8·5 |
| Loss by boiling ............... | 5 | ...... | ...... | ...... | 3·0 |

| | Jan. 15, 1856. | Feb. 27, 1856. | March 25, 1856. | April 29, 1856. | May 10, 1856. |
|---|---|---|---|---|---|
| Temperature of the water... | 46° F. | 43° F. | 44° F. | 50° F. | 51°·25 F. |
| | Grains in the gallon. | | | | |
| Total residue (including suspended matter) ............ | 26·58 | 22·22 | 28·148 | 20·736 | 19·67 |
| Suspended matter (clay and sand) ........................ | 1·179 | ...... | 3·729 | 1·34 | 1·179 |
| Organic matter (in solution) | 1·56 | 0·97 | 1·430 | 0·588 | 0·627 |
| Total dissolved salts ......... | 23·841 | ...... | 22·989 | 18·898 | 17·864 |
| Chlorine ........................ | 1·017 | 1·42 | 1·90 | ...... | 0·702 |
| Sulphuric acid ............... | 2·572 | ...... | ...... | 2·108 | 1·732 |
| Lime ............................ | ...... | ...... | 10·91 | 8·533 | 7·493 |
| Magnesia ...................... | | | | | |
| Hardness before boiling ... | 13·2° | 13·5° | 11·5° | 12·2° | 12·3° |
| Hardness after boiling ...... | ...... | ...... | 5·5 | | |
| Loss by boiling ............... | ...... | ...... | 6·0 | | |

N.B. The samples were collected in every case at 9 o'clock A.M.

From which the following average composition may be deduced :—

TABLE II.—Average Composition of the Thames Water at Kingston for the year.

|  | Maximum. | Minimum. | Mean. |
|---|---|---|---|
|  | In grains in the gallon. | | |
| Total impurity ......... | 28·148 | 18·37 | 23·488 |
| Suspended matter ...... | 4·41 | 1·17 | 3·034 |
| Organic matter ......... | 1·63 | 0·55 | 1·050 |
| Dissolved salts ......... | 22·108 | 16·65 | 19·404 |
| Chlorine ................. | 1·90 | 1·008 | 1·213 |
| Or Chloride of sodium (common salt)...... | 3·13 | 1·66 | 1·998 |
| Sulphuric acid ......... | 3·87 | 2·065 | 2·633 |
| Lime .................... | 10·91 | 6·487 | 7·884 |
| Hardness .............. | 13°·5 | 11° | 12°·05 |

But during a part of the time the analyses were repeated more frequently; in fact, during a period of about a month no less than seven analyses were made, which are exceedingly interesting, as showing to what extent the chemical composition may vary during that short space of time.

TABLE III.—Analyses of the Thames Water at Kingston, between January 21 and February 27, 1856.
(Direct experimental results.)

|  | Jan. 21, 1856. | Jan. 25. | Jan. 28. | Feb. 4. | Feb. 12. | Feb. 20. | Feb. 27. |
|---|---|---|---|---|---|---|---|
| Temperature of the water. | 43°·5F. | 44° | 39°·5 | 35° | 47° | 38°·5 | 43° |
|  | In grains in the gallon. | | | | | | |
| Total impurity ............. | 22·183 | 23·521 | 24·96 | 25·81 | 23·533 | 24·578 | 22·22 |
| Suspended matter ......... | 3·21 | 3·419 | 4·41 | 3·122 | 3·820 | 3·509 |  |
| Organic matter ............ | 1·052 | 1·112 | ...... | 0·917 | 0·553 | 0·8906 | 0·971 |
| Dissolved salts ............ | 17·92 | 18·990 | ...... | 21·772 | 19·160 | 20·178 |  |
| Chlorine .................... | 1·456 | 1·749 | ...... | 0·845 | 1·008 | ...... | 1·420 |
| Or Chloride of sodium (common salt) ...... | 2·399 | 2·882 | ...... | 1·392 | 1·66 | ...... | 2·337 |
| Hardness .................. | 11°·52 | 10°·8 | 11°·2 | 13° | 13°·15 | 13°·3 | 13°·5 |

From which the following averages may be deduced:—

TABLE IV.—Average Composition of the Thames Water at Kingston (from January 21 to February 27, 1856).

|  | Maximum. | Minimum. | Mean. |
|---|---|---|---|
|  | In grains in the gallon of water. | | |
| Total impurity ......... | 25·81 | 22·183 | 23·99 |
| Suspended matter ...... | 4·41 | 3·112 | 3·761 |
| Organic matter ......... | 1·112 | 0·533 | 1·665 |
| Dissolved salts ......... | 20·288 | 18·518 | 19·403 |
| Chlorine ................. | 1·749 | 0·845 | 2·594 |
| Or Chloride of sodium.. | 2·882 | 1·392 | 2·137 |
| Hardness .............. | 13°·5 | 10°·8 | 12°·5 |

To proceed with the composition of the water of the river at Chelsea: the following are the results of the complete analysis made at the beginning of the investigation.

The sample was collected by myself from the Chelsea Water-works Company's reservoir, at the time that it was being pumped in from the middle of the river on the 14th of June, 1855. The temperature of the water at the time was 62° F. (17°·5 C.), that of the air being 58° F. (14°·44 C.).

TABLE *a*.—Direct results of Experiments calculated in 1000 grains of the water.

| Exp. | Lime. | Magnesia. | Chlorine. | Sulphuric acid. | Alkaline chlorides. | Potassium. | Sodium. | Silica, alumina, and oxide of iron. |
|---|---|---|---|---|---|---|---|---|
| 1 | ·10814 | ·016 | ·07068 | ·04546 | ·1687 | ·0269 | ·0461 | ·0634 |
| 2 | ...... | ...... | ·0733 | | | | | |
| Mean. | ·10814 | ·016 | ·07199 | ·04546 | ·1687 | ·0269 | ·0461 | ·0634 |

| Exp. | Total soluble matter. | | | Total suspended matter. | | |
|---|---|---|---|---|---|---|
| | Inorganic. | Organic. | Total. | Inorganic. | Organic. | Total. |
| 1 | ·4382 | ·0148 | ·453 | ·5679 | ·1267 | ·6946 |
| 2 | | | | ·5103 | ·1267 | ·6470 |
| Mean ... | ·4382 | ·0148 | ·453 | ·5441 | ·1267 | ·6708 |

TABLE *b*.—Mean of the preceding calculated in grains in the gallon of water.

| | | |
|---|---|---|
| Suspended matter. | Inorganic . . | 38·107 |
| ... | Organic . . . | 8·873 |
| | Total . . . | 46·980 |
| Soluble matter. | Inorganic . . | 30·690 |
| ... | Organic . . . | 1·036 |
| | Total . . . | 31·726 |

| | |
|---|---|
| Lime . . . . . . . . | 7·5737 |
| Magnesia . . . . . . | 1·1205 |
| Chlorine . . . . . . | 5·0356 |
| Sulphuric acid . . . . . | 3·1838 |
| Alkaline chlorides . . . . | 11·815 |
| Potassium . . . . . . | 1·884 |
| Sodium . . . . . . . | 3·225 |
| Silica, alumina, and oxide of iron | 1·300 |
| Phosphoric acid . . . . | trace |
| Ammonia (as carbonate) . . | 0·033 |

TABLE *c.*—Saline Constituents in the gallon.

|  | grs. |
|---|---|
| Sulphate of lime . . . . . . | 5·411 |
| Carbonate of lime . . . . | 9·744 |
| Carbonate of magnesia . . . | 2·352 |
| Chloride of potassium . . . | 3·599 |
| Chloride of sodium . . . . | 5·471 |
| Carbonate of soda . . . . . | 2·475 |
| Organic matter . . . . . . | 1·036 |
| Silica, alumina, and oxide of iron | 1·300 |
| Phosphoric acid . . . . . . | trace |
| Ammonia (as carbonate) . . | 0·033 |
| Total soluble matter . . . | 31·421 |

Total residue obtained by experiment  31·726 grs.

Total Impurity in the gallon.

|  | Soluble. | Insoluble. | Total. |
|---|---|---|---|
| Inorganic . . | 30·385 | 38·107 | 68·402 |
| Organic . . . | 1·036 | 8·873 | 9·909 |
| Total . . . | 31·421 | 46·980 | 78·401 |

Hardness—

|  |  |
|---|---|
| Before boiling . . | 12°·62 |
| After boiling . . | 9°·00 |
| Loss by boiling . | 3°·62 |

The water being so much more impure here than at Kingston, the variations in composition during the year are more considerable, as shown by the following comparison of four analyses, made in June, October, and December 1855, and in May 1856.

TABLE V.—Analyses of the Thames Water at Chelsea during the year between May 1855 and May 1856.
(Direct experimental results.)

|  | June 14, 1855. | September 12, 1855. | December 29, 1855. | May 10, 1856. |
|---|---|---|---|---|
| Temperature of the water. | 62°·5 F. | 61° F. |  |  |
|  | In grains in the gallon of water. | | | |
| Total impurity ............ | 78·40 | 65·527 | 41·652 | 64·146 |
| Suspended matter :— |  |  |  |  |
| Organic ................. | 8·873 | 8·206 | 3·600 | 11·247 |
| Inorganic................. | 38·107 | 16·031 | 7·360 | 28·930 |
| Dissolved matter :— |  |  |  |  |
| Organic ................. | 1·036 | 0·67 | 1·65 | 1·349 |
| Inorganic............... | 30·385 | 40·62 | 29·092 | 22·620 |
| Chlorine .................... | 5·0356 | 11·14 | 1·667 | 1·956 |
| Sulphuric acid ............ | 3·1838 | 3·489 | 2·962 | 3·073 |
| Lime ....................... | 7·5737 | ......... | 8·675 | 8·719 |
| Magnesia ................. | 1·1205 | ......... | ......... | 1·189 |
| Hardness : — |  |  |  |  |
| Before boiling............ | 12°·62 | 12°·0 | 11°·9 | 13°·5 |
| After boiling ............ | 9·00 | 6·7 | ......... | 9·3 |
| Loss by boiling ............ | 3·62 | 5·3 | ......... | 4·2 |

From which the following average composition may be deduced :—

TABLE VI.—Average Composition of the Thames Water at Chelsea during the year.

|  | Maximum. | Minimum. | Mean. |
|---|---|---|---|
| Suspended matter :— | In grains in the gallon. | | |
|   Organic ............... | 11·247 | 3·60 | 7·423 |
|   Inorganic............... | 38·107 | 7·36 | 22·73 |
| Dissolved matter :— | | | |
|   Organic ............... | 1·60 | 0·67 | 1·16 |
|   Inorganic ............ | 40·62 | 22·62 | 36·62 |
| Chlorine ................... | 11·14 | 1·667 | 6·403 |
| Sulphuric acid ......... | 3·489 | 2·962 | 3·225 |
| Lime ..................... | 8·719 | 7·573 | 8·196 |
| Hardness ............... | 13°·5 | 11°·9 | 12°·7 |

The following points cannot fail to be remarked in considering these numbers :—

1st. The very much greater variations in the composition of the water here than at Kingston.

2nd. The great decrease in impurity generally, but especially in the suspended matter which takes place during the winter, and increase in the summer, above the mean for the whole year.

3rd. The extraordinary fluctuations in the quantity of common salt present, the chlorine varying from 11·14 grs. in the gallon in September 1855 to 1·667 in December of the same year.

4th. That the amounts of sulphuric acid, and of lime and magnesia salts, remain, however, much more constant.

But in order to present in the most striking form the *superiority* of the water at *Kingston* over that at *Chelsea* as a source for the *supply of London,* I have drawn up the following Table, exhibiting a—

TABLE VII.—Comparison of the *Mean Annual* Composition of the Thames Water at *Chelsea* and at *Kingston*.

|  | Chelsea. | Kingston. | Excess at Chelsea above Kingston. |
|---|---|---|---|
| Total impurity ......... | 67·933 | 23·488 | 43·445 |
| Suspended matter :— | | | |
|   Organic ...... 7·423 | } 30·153 | 3·034 | 27·119 |
|   Inorganic ... 22·73 | | | |
| Dissolved matter :— | | | |
|   Organic ............... | 1·16 | 1·05 | 0·110 |
|   Inorganic ............ | 36·62 | 19·404 | 17·216 |
| Chlorine ................... | 6·403 | 1·213 | 5·190 |
| Sulphuric acid ......... | 3·225 | 2·633 | 0·592 |
| Lime ..................... | 8·196 | 7·884 | 0·312 |
| Hardness ............... | 12°·7 | 12°·05 | 0°·65 |

Observe,—that the excess is chiefly in the suspended matter, and among the dissolved salts in the chloride of sodium; but that the increase in the *dissolved organic matter* is but slight, nearly the whole of the excess of organic matter separating with the mud in an insoluble state.

It must, however, be remembered, that in this Table the composition of the water as its exists in the river is compared, not the composition when *after filtration* it is supplied to the public.

But having previously investigated the effect of filtration upon the Thames water both at Chelsea and at Kingston*, I am enabled to draw a similar comparison of the composition of the water at these two points *after filtration*; which being likewise deduced from a large number of experiments, extending in fact over the whole year, may, I think, be relied upon as placing the question in an impartial light.

On referring to my former paper in the Philosophical Magazine, it will be seen that during nine months the composition of the water at Chelsea *after filtration* was as follows:—

|  | Sept. 12, 1855. | Dec. 29, 1855. | May 10, 1856. | Mean. |
|---|---|---|---|---|
| Total residue ............ | 41·29 | 24·153 | 22·85 | 30·03 |
| Suspended matter ...... | ......... | 1·403 | 2·285 | 1·84 |
| Soluble matter :— |  |  |  |  |
| Organic .............. | 0·674 | 1·889 | 1·349 | 1·30 |
| Inorganic.............. | 40·616 | 20·861 | 19·216 | 26·89 |
| Chlorine ............... ... | 11·14 | 1·546 | ......... | 9·59 |
| Sulphuric acid ......... | ......... | 2·372 | ......... | 2·37 |
| Lime ..................... | ......... | 8·402 | 8·426 | 8·41 |

TABLE VIII.—Comparison of the average Composition of the Thames Water at Chelsea and at Kingston *after filtration, i. e.* in the state in which it is supplied for public consumption.

|  | Mean composition at Chelsea after sand filtration. | Mean composition at Kingston after sand filtration †. | Excess of impurity in filtered water from Chelsea over that from Kingston. |
|---|---|---|---|
| Total residue ............ | 30·03 | 23·91 | 6·12 |
| Suspended matter ...... | 1·84 | 1·58 | 0·26 |
| Soluble matter :— |  |  |  |
| Organic .............. | 1·30 | 0·64 | 0·66 |
| Inorganic.............. | 26·89 | 21·69 | 5·20 |
| Chlorine .................. | 9·59 | 0·86 | 8·73 |

* See Phil. Mag. vol. xii. p. 23 (for July).
† Deduced from Table V. page 8 of former paper.

This last Table exhibits, I believe, a fair view of the question, and the numbers are in themselves so eloquent, that I leave them without any comment for the reader to make his own deductions from them.

In conclusion, I must observe that my friend Mr. Simpson is of opinion, from his own practical experience in the matter, that the results detailed above can only be considered as representing the mean composition for the year during which the experiments were made; that, in fact, from the much greater increase in the volume of the upland water which comes down the river in some years, when the rainfall is sometimes almost double what it was during the past year; and its diminution during others, when the rainfall does not exceed one-half what it then was,—there is every reason to believe that far greater fluctuations in composition sometimes occur than have been demonstrated by this investigation.

*Appendix to paper " On a peculiar Power possessed by Porous Media of removing Matter from Solution in Water."*

Allow me to correct an error in figures which occurred at page 2 of my former paper, relative to the dimensions and volume of water passing through the filters at the Chelsea Waterworks. The total filtering surface there is about three-fourths of an acre, or 32,670 square feet, and the filtration taking place at the rate of one foot per hour, yields about 204,187 gallons of filtered water per hour.

Since the publication of this paper, I have received the following letter from Mr. Robert Hunt, F.R.S., Keeper of the Mining Records; and the facts therein mentioned may serve to confirm my views respecting the peculiar power of porous media.

" Museum of Practical Geology, Jermyn Street,
July 12, 1856.

" Dear Sir,—I have read your paper with pleasure and profit. Allow me to give you two examples, within my own knowledge, confirmatory of your views.

" At Perran-porth, some six or seven miles north of Truro, the heaps of waste (*deads*) from the old mines contain considerable quantities of copper pyrites, which is, in the process of decomposition, converted into sulphate of copper. This salt is washed out by the rains, and the solution flows through the sands (*blown sand*) widely spread over that district. The sulphate of copper is separated by the sand, and the sand containing the copper is collected from time to time and sold to the copper smelters. Again, at Botallack Mine, the water which

filters through the rocks from the Atlantic Ocean in the levels which are worked out under the bed of the sea, is found to have lost much of its original saltness. These facts are at your service. I trust you will continue the interesting inquiry which you have commenced.

<div style="text-align:center">

"I am, dear Sir,

"Yours most truly,
</div>

"*H. M. Witt, Esq.*"                           "ROBERT HUNT."

---

XVI. *On the Resolution of Algebraic Equations of the Fifth Degree. By* JAMES COCKLE, *M.A., F.R.A.S., F.C.P.S., Barrister-at-Law, of the Middle Temple*\*.

81. THE last paper of my series on the Method of Symmetric Products terminated at p. 138 of this Journal for February 1854 (S. 4. vol. vii.). The numbering of the paragraphs is resumed here.

82. The problem of (79) may be solved thus. Let $Y_1$ vanish, and let the quintic in $y$ be

$$y^5 - 5By^3 - 5Cy^2 - 5Dy - E = 0. \quad . \quad . \quad . \quad (m)$$

Determine $\Theta$ and $\Omega$ from the quadratics

$$B\Theta^2 + (\Theta - C)\Omega = 0,$$
$$C(\Theta^2 + B^3) - (D + B^2)(B\Theta + \Omega) = 0,$$

and $Y_2$, $Y_3$ and $Y_4$ from the equations

$$Y_3 Y_4 = B, \quad Y_2 Y_3^2 = \Theta, \quad Y_3^5 = \Omega,$$

then the expression

$$\alpha Y_2 + \alpha^2 Y_3 + \alpha^3 Y_4$$

will give all the roots of $(m)$.

83. If, for the development of the right-hand side of the equation of (66), we employ the notation of (25) and the "epimetric" symbol of (69), and, further, if we make

$$c(Q_1^m Q_2^n) = c'(Q_1^m Q_2^n) - 5c''(Q_1^m Q_2^n),$$

where $c'$ is a symmetric and $c''$ an unsymmetric function, we have

$$c''(Q_1^4) = -5^{-1}\epsilon(x) = S_1(x_1^2 x_2 x_3),$$
$$c''(Q_1^3 Q_2) = 2S_1(x_1^3 x_2 x_3) - S_1(x_1 x_2^2 x_3^2),$$
$$c''(Q_1^2 Q_2^2) = 2S_1(x_1^3 x_2^2 x_3) + S_1(x_1^4 x_2 x_3),$$
$$c''(Q_1 Q_2^3) = 2S_1(x_1^3 x_2^2 x_3^2) + S_1(x_1^4 x_2^2 x_3),$$
$$c''(Q_2^4) = -5^{-1}\epsilon(x^2) = S_1(x_1^4 x_2^2 x_3^2).$$

---

\* Communicated by the Author. Mr. Cockle adds, that his "expression for $u_5$ (see his 'Fragment,' &c., Phil. Mag. for December 1853, p. 448, par. XX.) must be amended by the addition of the quantity $-\chi(-y_5)$ to it."

We also have

$$c'(Q_1{}^4) = \sigma(x), \quad c'(Q_2{}^4) = \sigma(x^2),$$
$$c'(Q_1{}^3Q_2) = \sigma`(x) - 5\Sigma(x_1{}^2x_2{}^2x_3)$$
$$c'(Q_1{}^2Q_2{}^2) = \sigma``(x) - 5\Sigma(x_1{}^2x_2{}^2x_3{}^2), \quad c'(Q_1Q_2{}^3) = \sigma```(x),$$

and

$$c(Q_1{}^4) = \sigma(x) + \epsilon(x), \quad c(Q_2{}^4) = \sigma(x^2) + \epsilon(x^2)$$
$$c(Q_1{}^3Q_2) = \sigma`(x) + 5\Sigma(x_1{}^2x_2x_3x_4) - 5S_1(x_1{}^3x_2x_3) + \Sigma(x).\epsilon(x).$$

The last equation is a consequence of that of (73). There are, of course, two more equations resembling the latter three.

I have elsewhere shown (see pp. 226 and 486 of vol. lii. of the Mechanics' Magazine) that when $\sigma(x)$ is a symmetric product, all such functions as $\sigma(x^2)$, $\sigma`(x)$, &c. are symmetric. The rule which I gave in (6) of p. 299 of vol. i. of the 'Mathematician' may, it would seem, be applied to the derivation of $\sigma`(x)$, $\sigma``(x)$, &c. from $\sigma(x)$.

For quintics wanting their second term $c(Q_1{}^3Q_2)$ is simpler in form, and equal to

$$\sigma`(x) + 5\Sigma(x_1{}^2x_2x_3x_4) - 5S_1(x_1{}^3x_2x_3).$$

84. Glimpses of a relation between different epimetrics have already presented themselves. With that of (72) may be classed the following :—

$$\frac{S_1(x_1x_2{}^2x_3{}^2)}{x_1x_2x_3x_4x_5} = S_1\left\{\frac{x_2x_5}{x_3x_4} + \frac{x_3x_4}{x_2x_5}\right\},$$

each side of which is a six-valued function. And there is also an inexhaustible mine of relations corresponding to that of (73).

85. By $S_1(x_1{}^mx_2{}^nx_3{}^p)$ I denote the epimetric

$$S_1\{x_1{}^m(x_2{}^nx_5{}^p + x_5{}^nx_2{}^p + x_3{}^nx_4{}^p + x_4{}^nx_3{}^p)\}.$$

86. Let

$$\{s_\alpha - \phi(x_5)\} \div (t_\alpha t_\beta t_\gamma t_\delta) = \Xi_\alpha;$$

then, from equations noticed in (57) and (58), we have

$$\Xi_1 = \frac{t_1}{t_2} + \frac{t_2}{t_4} + \frac{t_4}{t_3} + \frac{t_3}{t_1}, \quad \Xi_4 = \frac{t_1}{t_3} + \frac{t_3}{t_4} + \frac{t_4}{t_2} + \frac{t_2}{t_1};$$

in other words,

$$\Xi_1 = \psi(t), \quad \Xi_4 = \Xi_1\binom{14}{..} = \psi(t^{-1}).$$

We also find

$$\Xi_2 = \Xi_1\binom{12}{..} = \psi'(t), \quad \Xi_6 = \Xi_1\binom{34}{..} = \psi'(t^{-1})$$

and

$$\Xi_3 = \Xi_1\binom{13}{..} = \psi''(t), \quad \Xi_5 = \Xi_1\binom{24}{..} = \psi''(t^{-1}).$$

The first pair of relations may be changed into

$$\Xi_1 = u_1 + u_2 + u_3 + u_4 = v_1 + \frac{v_3}{v_1} + \frac{v_2}{v_3} + \frac{1}{v_2}$$

$$\Xi_4 = \Sigma(u^{-1}) = \Sigma(u_1 u_2 u_3) = \frac{1}{v_1} + \frac{v_1}{v_3} + \frac{v_3}{v_2} + v_2,$$

or otherwise varied, as may the other two pairs.  These results
will perhaps throw light upon the question of the solvibility of
the equation in *s*.  If $\epsilon(x)$, $\epsilon'(x)$, &c. can be determined, the
general equation of the fifth degree admits of finite algebraic
solution.

76 Cambridge Terrace, Hyde Park,
June 13, 1856.

XVII. *On a substance named Bleiniere in modern Treatises on
Mineralogy.* By H. J. BROOKE, *F.R.S.*[*]

THIS mineral, which is described as having been hitherto
met with only at Nertschinsk in Siberia, has been recently
found in Cornwall by Mr. Richard Talling, of Lostwithiel.

It is uncertain from what ores the Nertschinsk mineral has
been produced, but the Cornish variety appears to have resulted
from the decomposition of Jamesonite.

The Siberian mineral appears to vary considerably in the pro-
portion of its constituent elements.

Hermann found the specimen which he analysed to be com-
posed of—

Protoxide of lead . . . 61·83
Antimonic acid . . . 31·71
Water . . . . . . 6·46

Other specimens were found by Pfaff and Hermann to contain
variable proportions of arsenic acid; and a reddish-brown spe-
cimen from Siberia, recently examined by Mr. Allan Dick, at
the Museum of Practical Geology in Jermyn Street, contains a
large proportion of arsenic without any trace of antimony.   Dif-
ferent portions of the Cornish mineral have also been analysed
by Mr. Dick, and are found to vary in their composition so much
as to render it probable that no two specimens would furnish the
same results.

One portion consisted of—

Oxide of lead . . . . (PbO) 40·73
Oxide of antimony . . (SbO$^5$) 47·36
Water . . . . . . . . 11·91

* Communicated by the Author.

It seems to be a mixture of hydrated antimonite of lead, with variable proportions of the oxides of lead and antimony.

Since the examination of the Cornish ore by Mr. Dick, it has been analysed by Dr. Heddle with the following results, which agree with the supposition of its being a variable mixture of its component parts :—

|  | I. | II. |
| --- | --- | --- |
| Oxide of lead . . | 47·045 | 46·68 |
| Antimonious acid . | 42·216 | 42·44 |
| Water . . . . . | 11·497 | 11·98 |

A brown variety consisted of—

| | |
| --- | --- |
| Oxide of lead . . . . . . . | 43·94 |
| Antimonious acid and Antimony ochre } . . . | 46·70 |
| Water . . . . . . . . | 6·625 |

---

## XVIII. *On the Theory of the Origin of Slaty Cleavage.* *By* H. C. Sorby, *F.G.S.*[*]

I SHOULD not now have trespassed on the pages of the Philosophical Magazine, if it had not been that in Dr. Tyndall's paper on the comparative view of the cleavage of crystals and slate-rocks, published in its last Number, there is a most serious misapprehension of my opinions on that subject. In alluding to these, at page 44, he says, " I cannot accept his explanation of slate-cleavage. I believe that even if these plates of mica were wholly absent the cleavage of slate-rocks would be much the same as it is at present. I will not dwell upon minor facts,—I will not urge that the perfection of the cleavage bears no relation to the quantity of mica present ; but I will come at once to a case which to my mind completely upsets the notion that such plates are a necessary element in the production of cleavage." Also somewhat further on he speaks of "the plates of mica assumed in Mr. Sorby's theory." Now this statement of my views respecting the origin of slaty cleavage is so entirely different from what is really the case, that I think it would not be proper for me to let it pass uncorrected. In proof of this I need only refer to my paper on the cleavage of the Devonian limestones of Devonshire, published in this Magazine (January 1856, 4th series, vol. xi. p. 20). I there describe that of a considerable number of specimens, including all the principal varieties I have seen in the district, and yet in *no case* do I attribute it to the presence of *mica*; and do not even use that word in the whole paper, except in saying, at page 23, that I do

not see why those varieties of the limestone, which have been changed into dolomite, should not be called *metamorphic*, because they do not contain mica or such other minerals as occur in rocks to which that term is usually applied.   I would also particularly call attention to what I state in describing figs. 5 and 6, where I show that the difference in the structure of non-cleaved and cleaved, fine-grained organic clays, consisting, now, almost entirely of minute crystals of calcareous spar with no mica, or fragments of any other such material, can be explained on the theory of mechanical compression and change in dimensions.

I think this will be sufficient to prove that Dr. Tyndall has unfortunately misunderstood my meaning, in concluding that I considered slaty cleavage to be invariably due to the change in the position of flakes of *mica*; and I much regret that this should have occurred, for it has led him to describe a number of most interesting experimental facts as if they disproved my theory, whilst, on the contrary, I hail them with the greatest pleasure as confirming it in the most remarkable manner.

What I chiefly wish to impress on the attention of geologists and physicists is, that slaty cleavage is due to mechanical causes; that cleaved rocks are compressed rocks; and that the compression in general has not only changed the arrangement of the unequiaxed particles of which they are composed, but in some cases has also altered their form.   There are scarcely any rocks whose particles are not thus unequiaxed, and I must still maintain that, other circumstances being the same, those have the best cleavage that are composed of particles whose length and thickness differ most.

The example of wax described by Dr. Tyndall (Phil. Mag. July 1856, p. 44) is indeed most excellent.   If a small quantity, so melted on a piece of glass as to be sufficiently thin, be examined when cold with a magnifying power of about 400 linear with polarized light, it is seen to be composed of prismatic crystals usually about $\frac{1}{1000}$ to $\frac{1}{3000}$th of an inch long and $\frac{1}{3000}$ to $\frac{1}{10000}$th broad, being almost invariably very unequiaxed.   These are arranged in imperfectly radiating groups, and interlace promiscuously in all directions; and this is just such a structure as, according to my views, would, if it was compressed, give rise to a cleavage of so perfect a kind that I can readily believe that the presence of larger particles would only impair it.   Similar remarks would also apply in the case of white-lead and graphite, as mentioned by Dr. Tyndall (ibid. p. 46); for carbonate of lead is seen with the microscope to consist of minute prismatic crystals, and graphite of thin scales.

Though I do not consider mica a necessary element in the production of cleavage, yet still I think it is very much more

commonly instrumental in giving rise to it than is generally supposed. For instance, the slates of Penrhyn and Llanberis contain about half their bulk of mica, which cannot indeed be seen with the naked eye, because the flakes are on an average only $\frac{1}{1000}$th of an inch long and $\frac{1}{10000}$th thick, and but few are much larger. When, however, thin sections of these rocks are examined with a microscope these flakes are most distinctly seen, especially in the black slate of Llanberis village, which to the naked eye is just like a fine-grained black shale, scarcely at all micaceous in the usual application of that term. Chemical analysis also fully bears out this conclusion; but I may perhaps as well state here that the characters of this mica appear to me to indicate that it was not deposited in its present condition, but agree very much better with the supposition of having been formed by a peculiar transformation of a deposit of clay, of a different kind to that which has occurred in mica-schist and rocks usually called metamorphic, as I shall describe in a paper I hope to be able to publish before long, on the chemical and physical structure of the older rocks. This size of the grains in these slates is then very similar to that of the crystals of wax, or in other words, the grain is as fine as that of wax. Their particles also are of such an unequiaxed character as completely agrees with their remarkably perfect cleavage, and I could only attribute any other view of the subject to trusting to the natural appearance of the rock, without examining a thin section with suitable microscopical means.

In conclusion I wish particularly to call attention to the fact, that in forming a theory of the origin of the cleavage of slate-rocks, we must not rest satisfied with one that merely accounts for the splitting—it must be such as will explain their actual ultimate constitution—the mountains *must indeed be examined with the microscope;* for it shows that the cleavage is not mere joints or weak places, which do also occur most abundantly, but chiefly depends upon a structure extending to the mechanical arrangement of the ultimate grains of which they are composed.

---

## XIX. *Observations on the preceding Paper.* By JOHN TYNDALL, *F.R.S. &c.*

THE history of the paper which has called forth the foregoing remarks is briefly as follows. In examining the influence of structure upon the magnetism of bodies, I had occasion to make numerous experiments on slate rocks, and in this way the desire arose on my part to make myself acquainted with the agencies by which the structure of such rocks was produced.

During an accidental meeting with Mr. Woodward, the geologist, in the pass of Llanberis, on Saturday the 30th of September, 1854, my longing for information on this head made itself manifest, and soon after my return to London I was gratified by the receipt of Mr. Daniel Sharpe's papers on cleavage, sent to me by the lamented author; and of a paper by Mr. Sorby, sent to me, I believe, by Mr. Woodward. All these papers interested me in a high degree. Returning to my experiments in the autumn of 1855, and making use, as on former occasions, of pressure as a means of modifying the magnetic force, I was gradually led to a theory of slaty cleavage different, as I conceived, from that proposed by Mr. Sorby. I had previously seen at Glasgow a specimen of the cleavage produced by Mr. Sorby, by means, as he considered, of oxide of iron scales; but that which I had succeeded in producing, *without such scales*, being incomparably finer, I was naturally led to doubt the influence which he assigned to them.

The foregoing remarks, however, represent me as labouring under a most serious misapprehension of Mr. Sorby's opinions; my statement of his views " is so entirely different from what is really the case, that it would not be proper to allow it to pass uncorrected." Nobody can regret this misapprehension more than I do; and all I intend to do at present is to show, by quotations from the paper to which I have referred, the basis of my interpretation of Mr. Sorby's views. Whether these quotations will justify my interpretation in the eyes of the reader I know not; but I have at least the satisfaction of knowing that I by no means stand alone in the meaning which I have attached to Mr. Sorby's language.

The paper which contains Mr. Sorby's theory of the cleavage of slate rocks is to be found in the Edinburgh New Philosophical Journal, vol. lv. p. 137. At the commencement the writer states as follows:—

P. 137.—" The examination of thin sections of slate rocks with high powers, and a comparison with those of similar mineral composition not possessing cleavage, have led me to form a theory to account for their difference of structure materially different from any yet propounded, and which, in my opinion, not only does so most satisfactorily, but also explains perfectly every fact that I am acquainted with connected with the subject."

What this theory is the following extracts will explain:—

P. 142.—" Usually, besides *mica*, there is found in good roofing-slate, like that at Penrhyn, a certain proportion of decomposed felspar, a few minute grains of quartz sand, and granules of phosphate of iron. The red tint is produced by the presence of very numerous minute crystals of peroxide of iron, and the dark by those

of pyrites. From such slate there is every gradation to those containing *little or no mica,* but made up of more or less fine quartz sand and decomposed felspar in very variable proportions, *but these have only an imperfect cleavage.* Other slates, as is well known, contain much chlorite and other minerals. On the present occasion I shall chiefly confine myself to the consideration of such slate as has a perfect cleavage.

"If a thin section of a rock not having cleavage be examined, which has a similar mineral composition to those which, when having it, form good slates, it will be seen that the arrangement of the particles is very different. For instance, the well-known Water-of-Ayr stone has no cleavage, but shows more or less of bedding. It consists of *mica* and a very few grains of quartz sand imbedded in a large proportion of decomposed felspar; the peroxide of iron being collected to certain centres, and having the characters of peroxidized pyrites. The *flakes of mica* do not lie in the plane of bedding, but are inclined tolerably evenly at all angles, *so that* there is no definite line of structural weakness independent of that due to bedding; which results chiefly from alternations of layers of somewhat different composition, and not from the arrangement of the ultimate particles. This is, however, totally different in a rock of similar composition having cleavage. If a section be examined, cut perpendicular to cleavage, in the line of its dip, it will be seen that though some of the minutest *flakes of mica* lie perpendicular to the cleavage or at high angles to it, by far the larger part are inclined at low, so that the majority lie within $20^\circ$ on each side of it. In fact they are most numerous nearly in the plane of cleavage, and gradually but rapidly diminish in quantity in passing to higher angles; so that there are twenty times as many nearly in the plane of cleavage as at $45^\circ$ to it, and very few at $90^\circ$. Where a section is examined, cut perpendicular to cleavage in the line of the strike, it is seen that the arrangement is similar, but there is not near so rapid a diminution of the members in passing from the line of cleavage, so that there are comparatively several times as many more inclined at about $45^\circ$ to it than when the section is in the line of dip, and those at still higher angles are also much more numerous. In a section in the plane of cleavage, but few flakes are cut through so as to have a greatly unequiaxed form; but they are similarly arranged with respect to the line of dip, though not in so marked a manner. It is not merely the larger *flakes of mica* that are thus arranged, but the whole of those unequiaxed particles which existed in the rock before the cleavage was developed.

"When a cleavage crack in the thin sections is examined, it is clearly seen that the cleavage is due to the above-described arrangement of the particles, which it follows most perfectly; not passing straight forwards, but turning about according to the manner in which the ultimate particles lie in every part. *It therefore appears that the fissile character of slate is due to a line of structural weakness, brought about by the manner of arrangement of the ultimate unequiaxed particles.* The natural cleavage cracks, of course, bear the same

K 2

relation to this arrangement as those so often seen in many crystalline bodies do to that of their ultimate atoms. They appear, in general, to have been mainly due to meteoric agencies, their position having been determined by the structural weakness. In accounting, then, for so-called slaty cleavage, it is only requisite to show how such particles could have had their position so changed that their arrangement should be altered from that found in rocks not having cleavage to that in those having it, which explanation must of course be such as would agree with every other fact connected with the subject.

" Now I trust I have already shown that there is abundance of evidence to prove that rocks having slaty cleavage have been greatly compressed in a line perpendicular to cleavage, and elongated to a certain extent in the line of its dip. Taking for the amount of these changes those I have already mentioned for the slate of Penrhyn and Llanberis, it is easy to calculate mathematically what would be the arrangement of the unequiaxed particles in such a rock as Water-of-Ayr stone if its dimensions were so changed. Supposing that $A =$ the angle of inclination of the longer axes of any unequiaxed particle to the line along which the minimum elongation would occur, and that $a =$ this angle after it had taken place, we should have, perpendicular to cleavage in the line of dip, $\tan a = \dfrac{\tan A}{6}$; in that of strike,

$\tan a = \dfrac{\tan A}{3 \cdot 75}$; and in the plane of cleavage, $\tan a = \dfrac{\tan A}{1 \cdot 6}$. From these relations it necessarily follows, that the particles would then be arranged in precisely such a manner as is seen to be the case in such a rock having cleavage, the agreement being most perfect in every particular, both in kind and amount, as seen in sections cut in each direction.

" Though such calculations may be fully relied on, yet to satisfy myself that they were correct, I have tested them by actual experiment. Having mixed some scales of oxide of iron with soft pipe-clay in such a manner that they would be inclined evenly in all directions, like the *flakes of mica* in Water-of-Ayr stone, I changed its dimensions artificially to a similar extent to what has occurred in slate rocks. Having then dried and baked it, I rubbed it to a perfect flat surface, in a direction perpendicular to pressure and in the line of elongation, which would correspond to that of dip of cleavage, and also, as it were, in its strike and in the plane of cleavage. The particles were then seen to have become arranged in precisely the same manner as theory indicates that they would, and as is the case in natural slate; so much so, that so far as their arrangement is concerned, a drawing of one could not be distinguished from that of the other. Moreover, it then admitted of easy fracture into thin flat pieces in the plane corresponding to the cleavage of slate, whereas it could not in that perpendicular to it."

If the reader feel sufficient interest in the question, I would invite him to turn to my paper in the last Number of this

Journal, and decide for himself the extent to which I have erred in my statement of Mr. Sorby's opinions. My experiments on this subject were all finished on the 27th of October, 1855, and at that time the paper from which the above citations are drawn was the only one which, as far as I am aware of, Mr. Sorby had ever published on the subject of cleavage. From November 1855 to June 1856 I was far too busy with other matters to be able to devote any attention to the subject of cleavage, so that the lecture which has been the cause of this discussion was merely the utterance of views to which experiment had led me upwards of seven months before.

My special object in the lecture was to account for the cleavage of ordinary roofing-slates, like those of Bangor and Borrodale— of such slates, in fact, as Mr. Sorby had in view when he propounded his theory. In the above critique, however, Mr. Sorby refers exclusively to a paper on the cleavage of Devonian limestone, drawn up, I believe, at my suggestion, and published in the Philosophical Magazine for January 1856. "In no case," he observes, "do I attribute it [the cleavage] to the presence of *mica*; and do not even use that word in the whole paper, except . . . . at page 23." Mr. Sorby surely cannot imagine that I attach any particular value to mica *as such.* It is *shape*, and not *composition*, that affects the question—"elongated particles," as I have somewhere expressed it in my lecture. And if mica occupies a prominent place there, it is because Mr. Sorby himself has given it that prominence, as the foregoing extracts prove, while accounting for the cleavage *of the particular kind of slate which formed the subject of my discourse.* But even in this paper on the Devonian limestone, though mica is absent, it has its equivalent in elongated particles of sand and organic fragments, which perform the same functions in the limestone as the mica does in the Welsh and Cumbrian slate. So evident, indeed, does this equivalence appear to Mr. Sorby himself, that he makes use of the same experiment with the oxide of iron scales to illustrate both the action of the plates of mica in common slate, and of the "unequiaxed particles" in Devonian limestone. In proof of all this I refer the reader to the papers of Mr. Sorby; and I would here once for all express the hope that he will accept none of my statements without this verification; otherwise I may, although quite unwittingly, lead him into error.

Figs. 5 and 6, to which Mr. Sorby calls particular attention, and which occur towards the conclusion of his observations on the cleavage of Devonian limestone, refer to a cleaved organic clay composed almost entirely of minute crystals of calcareous spar, which "appears as though the compression indicated by the joints of the encrinites and larger crystals, had affected the

smallest, constituting the ultimate structure of the rock." This case is so like that of iron, adduced in illustration of my views, that had it occurred to me, I should undoubtedly have referred to it in my lecture. To this class of facts Mr. Sorby ingeniously endeavours to attach my experiment with the wax. When a thin stratum of this substance, melted on a plate of glass and permitted to solidify, was examined by the microscope, he found it to be composed of unequiaxed prismatic crystals. Without entering into the question as to the amount of cohesion between these crystals, I would say let them be chopped up into microscopic mincemeat, let the mass be rubbed away by emery, or pared into the finest shavings by glass; let it be pounded, crushed,—in short let all mechanical means be applied to abolish this crystalline structure; let the wax be kneaded, as in my experiments, into dough, and submitted to pressure,—the same perfect cleavage will be produced. To one and the same cause I have attributed the cleavage of such wax, and that of the slates of Wales and Cumberland. Mr. Sorby deduces from his experiments, that the volume of slate-rocks has been reduced one-half by the pressure which produced the cleavage. Without expressing any opinion as to the probability of this conclusion, let the structure of such a mass of fine slate mud, before compression, be considered. Its water has drained away from it, oozing through it where it encountered least resistance, and leaving what, to my mind, presents an innumerable assemblage of little mud masses clinging together at some parts and separated at others. The effect of pressure upon such a mass must manifestly be to develope cleavage. The presence of unequiaxed particles may aid in producing this result, but the finer the mud, the more in fact it approaches to the wax in structure, the more perfect, to my mind, will the cleavage be.

A day or two ago the presence of cream* cheese upon the table of a hotel suggested to me the idea of trying whether the squeezing of the mass had not developed in it a kind of cleavage. I was much pleased to find this to be the case. The experiment was performed by inserting the knife a quarter of an inch into the cheese and tearing the latter asunder. It is still simpler and better to pull the mass asunder with the fingers. The cheese yielded freely along the planes perpendicular to the direction of pressure, but not across these planes. If this can be referred to the compression of crystals, it is something won for science; if not, it illustrates the action by which I suppose the structure of ordinary roofing-slate to have been produced.

* The term 'cream' ought, properly speaking, to have the negative sign prefixed; the cheese was made with milk from which the cream had been *withdrawn.*

To Mr. Williams of Basingstoke I have been recently indebted for some very interesting specimens of iron plates, peeled off by the compressing and sliding action of railway trains. The specimens exhibit the laminated structure in a very striking degree. "Practically," says my intelligent correspondent, "it is a constant fault in rails : the men who are always handling and taking them up say that it is worst near the stations, where the break acts ; but sometimes they find one here and there upon the main road —not very often." One of the specimens sent to me was of this latter class, and it had probably been submitted to the rolling of the trains for a period of eighteen years. In addition to the pressure, which of itself causes the granules of iron gradually to spread out, and thus tends to destroy their natural cohesion, when the wheel *slides* it has a tendency to displace the upper surface of the rail in the direction of motion. Suppose the rail to be divided into distinct strata, each of these being held to its neighbours by the cohesion of the iron. Whenever a train passes, a slight displacement occurs, and every such displacement tends to rupture the cohesion of the particles which hold each pair of strata together. The act is repeated, perhaps thousands of times, until finally the rail yields at the place where, owing to crystalline or mechanical structure, the force which holds the strata together is weakest. When slate mud is compressed, its different layers cannot be expected to move laterally with exactly the same velocity ; they slide over each other, and this action, as in the case of the iron, must also tend to weaken the cohesion of the layers and to facilitate the cleavage of the mass.

With regard to Mr. Sorby's concluding paragraph, I confess that my sole object was to account for the "splitting" of slate-rock. The facts observed by Phillips and Sharpe, and added to in so important a degree by Mr. Sorby's researches, lead to a conclusion, first I believe announced by Sharpe, and assented to by all who have observed the facts, that the masses have been subjected to pressure in a direction at right angles to the planes of cleavage ; and the sole question to be decided was, whether this pressure was sufficient to produce the cleavage. Both Mr. Sorby and I say yes,—but on different grounds ; which of us is right (perhaps each of us has a little reason on his side) others must determine, and to them I now resign the consideration of the question.

## XX. *On certain Phænomena of Electrical Discharge.* *By* Sir W. Snow Harris, *F.R.S.*

*To the Editors of the Philosophical Magazine and Journal.*

Gentlemen,

I HAVE duly considered Dr. Riess's notice of my memoir in your May Number, "On Electrical Discharge." I regret exceedingly that it should have been treated by Dr. Riess in so dictatorial and intolerant a spirit. It will be my endeavour, however, in the few concluding observations I have to make, to avoid every thing calculated to provoke angry discussion, and confine myself strictly to facts and to the scientific merits of the question at issue between us, without any discourteous comment on the several personal animadversions and reflections on me, in which Dr. Riess has thought fit to indulge.

Dr. Riess says that I had "long since set up a law upon the dependence of the electrical heat upon the charge of the battery," and that he "ascertained this law to be incorrect, and set up another in its place." All this, however, as it here stands, is mere assertion, worth nothing as scientific evidence in reply to the facts and phænomena set forth in my memoir. According to my view of the question, Dr. Riess has quite failed in his demonstration, which I conscientiously believe to rest entirely upon defective hypothetical assumptions, at variance with the known laws of ordinary electrical action. The essence of what I advanced and what I still maintain is this, that you cannot extend your electrical battery by adding to the number of its jars, without at the same time increasing the resistance to the subsequent discharge of the accumulation; consequently a given quantity of electricity will experience more resistance when discharged from an increased number of jars, than when discharged from a smaller number; so that you *cannot* take the resistance as constant. But, as I have shown, if you extend your battery-surface without division, as in accumulating the charge upon a very large jar instead of a very small jar, then in using the same charging rod you have the resistance nearly the same in each; now in this case you find the effect on the wire of the thermo-electrometer invariable. What then becomes of Dr. Riess's formula $F = \dfrac{Q^2}{s}$, taking $s$ to indicate the increased surface? Why the surfaces may be as $1 : 2$, and yet the effect of the discharge of a given quantity of electricity the same. According to Dr. Riess, the density of the accumulated electricity would be in such a case twice as great in the small jar; so that if the effect were as he says, "proportional to the product of the quantity by its density," the same quantity discharged from a jar of half the surface should

produce twice the effect; whereas the effect is unchanged. Then again I am informed that my paper is full of errors, that without the necessary knowledge, I have deduced results unintelligible both to Dr. Riess and others, and he is "not aware that any one, either here or in England, had found these results more intelligible than he had done." All these things are very easily said; but here is an extract from Faraday's Researches, 12th series, 1363: "Many philosophers have examined the circumstances of this limiting action in air, but as far as I know none have come near Mr. Harris as to the accuracy with, and the extent to, which he has carried on his investigations." This, be it remembered, refers to the particular method of research so pointedly objected to by M. Riess, "if his memory is correct." That my researches may be ill understood by Dr. Riess is possible, and yet that may not be altogether my fault; it may equally arise from some source of misapprehension by him. We have an instance of this in his late notice of my memoir, in his remark relative to the resistance to discharge as depending on the transfer of the accumulation from the glass to its coating, and the accumulation of a given quantity of electricity upon an increased number of jars. These sources of resistance are evidently new to Dr. Riess, and so far as I can see, have never entered into his calculation; now *I have* "ascertained by experiment" the fallacy of the signification which Dr. Riess attaches to his symbol, as seen in my former communication.

Again, Dr. Riess observes, "*It is not true* that I ascribed the unsuccessful experiments of Sir W. Harris to the faulty arrangement of his thermometer." I think it will be allowed, that before employing so strong an expression, something like certainty upon the point in question should have been established. Well! here is an extract from Dr. Riess's paper (*Annales de Chimie* for 1838, tome lxix. p. 116): "*Il faut attribuer à la construction imparfaite du thermomètre, que les expériences de M. Harris sont restées infructueuses;*" and yet I am told that it is "not true" that M. Riess had referred what he calls my unsuccessful experiments to the imperfection of my instrument*.

Nothing that Dr. Riess has advanced relative to Kinnersley's thermometer, with the whole detail of which I have been long familiar, at all serves to convince me that my instrument in all its generality was not an entirely new instrument at the time it was first produced, at least so far as it is possible to produce a perfectly new instrument; and I again repeat, that the main object of all the primitive air-electrometers, was the explosive violence of the electrical discharge in a confined space of air. Kinnersley's casual introduction of various substances

* See also De la Rive, *Traité de l'Electricité*, vol. ii. p. 154.

into his cylinder, I consider as a mere coincidence, quite a secondary affair; the experiments came to nothing, and the "Immortal" Franklin gave these experiments with the "wet thread," "blade of green grass," &c., fairly quoted by Dr. Riess, no encouragement whatever, as resulting from heat, and so the whole matter dropped; and it has not, so far as I know, been ever revived until the present time. Now Dr. Riess labours to show that my instrument was a studied copy of these old instruments, Kinnersley's more especially, much in the same way as he improved upon Behrens' electroscope, or copied my instrument; whereas at the time I first invented my instrument, I can most rigidly affirm that I had not the shadow of Kinnersley's air-electrometer in my mind. As I have stated, it originated at a pressing moment, in a necessity for demonstrating experimentally to the Naval Department of the Government, and the Scientific Commission appointed to investigate and report on my method of securing the Royal Navy from lightning, the relative superiority of copper for that purpose over iron, and some other metals; at that time no very exact experiments had appeared, more especially with the ordinary electrical discharge. Now Dr. Wollaston, one of the most distinguished philosophers in Europe of that day, did not consider the experiments I made with this instrument "unintelligible," as Dr. Riess says he has found them. On the contrary, in reply to an inquiry whether he saw "any danger or liability to objection in the method proposed by Mr. Harris, of carrying off lightning from ships," &c., he observes, "Mr. Harris appears to me to be well acquainted with the subject, and fairly to estimate the operation of the element with which we have to contend," &c. I find it of course somewhat painful to be obliged to refer to such quotations; but in justice to myself I think I am called on to expose, by an appeal to facts, how ill-supported are Dr. Riess's assertions of my want of knowledge, &c. of the question I have undertaken to discuss, and the "unintelligible" nature of my experiments, both to himself and "others."

With respect to Dr. Riess's assertion, that I must be unacquainted with the use of the thermometer, being still "unaware of the necessity of employing a calculation," &c., I think it may be seen by reference to what I stated, that I neither asserted nor denied the necessity of "calculation." I in no way meddled with the very loose ground on which Dr. Riess builds his conclusion, and I took very especial care to explain my view of the operation of the instrument. In the present imperfect state of our knowledge of the causes of heat and electricity, and of our equally imperfect apprehension of the precise way in which my thermo-electrometer operates, I cannot but conclude that the

application of hypothetical formulæ as a means of correcting its indications, is a very unsafe and precarious course. Dr. Riess nowhere proves the application of the doctrines of specific heat to the operation of the instrument, nor does he in any way pretend to show how it operates, except upon an assumption. I by no means question Dr. Riess's analytical calculation, or the skill and ability it displays : I question its applicability to the nature of the experiment. Supposing it should turn out, as is by no means unlikely, that the expansive action is a sort of secondary action, varying merely with the evanescent effect on the wire, independent of the heating effect, but with which it may have some peculiar connexion,—what then becomes of Dr. Riess's elaborate conclusions ? Franklin, for example, evidently imagined that the expansive effects observed by Kinnersley were the result of vapour produced by the shock, or of a sort of smoke emitted by the wires, and so far did not assent to the heat doctrine upon which Dr. Riess now stakes his whole process of research.

From some interesting experiments instituted since my memoir on this subject appeared, I am led to conclude that we have yet a vast deal to learn relative to the precise operation of this instrument, and that the view I took of it, as depending on the momentary mechanical expansion of a small column of air immediately surrounding the wire, is substantially correct. But until all this be well ascertained, I think it a much safer course to trust to an empirical experimental examination of the immediate indications of the instrument, and observe the general laws of its operation by comparison with well-known laws of electricity. Hence I pretend to say nothing about the " necessity " or otherwise, of some calculation for rendering " the data capable of comparison."

Now as to my " unit measure," which Dr. Riess treats so slightingly : I believe the time is not far distant when he will be compelled in candour to change his opinion upon that question. I have lately submitted to the Royal Society a series of crucial experiments showing the perfect accuracy of this instrument as a measure of quantity, which I believe Dr. Riess will have great difficulty in disproving ; nor do I stand alone in my interpretation of this question, however meanly Dr. Riess may profess to think of it ; and I am quite willing to stake the value of the differences existing between Dr. Riess and myself upon the soundness of our respective scientific views of this question alone.

Faraday, in considering the question of my unit measure, thus expresses himself : " Suppose the unit-jar has about one-tenth the electrical capacity of the large jar, and that being

charged up to its discharging point, it contains *ten* of positive electricity; then these ten will pass on into the large jar as a discharge spark, and none will remain *within* the unit-jar. Now the conductor of the machine, the outside of the unit-jar, and the ball and wire of the large jar, will all appear positive to a carrier ball: but when the machine is turned, although a rise in positive condition will gradually take place on all the surfaces, still the mutual relation of $n$ and $n'$ (that is, the exploding balls) to each other, will be the same as before, and the mutual relation of the inner and outer coating of the unit-jar will be to each other *absolutely as before*, for no external relation can alter their mutual relation, though it may affect the outer coatings, both of the large jar and the unit-jar. Whenever a spark does pass from $n$ to $n'$, the electricity passing must be equal; because the inductive relations of the coatings to each other through the glass, and the like relations of the balls $n$, $n'$ to each other, remain absolutely the same. This is, as I think, a rigid consequence of the principles of inductive action." This quotation, independent of the great authority from which it is drawn, may perhaps help Dr. Riess to a better appreciation of the question, and serve in some degree to correct his notion, that my results, " so far as he is aware," are equally unintelligible in England as they are to himself " and others " in Germany.

I am unwilling to extend my observations further on this present occasion, not having any predilection for a controversy which seems to border on so much painful matter of difference. I had, certainly, when I wrote the paper which appeared in your May Number, not the least intention of offering to Dr. Riess any personal affront. 1 was perhaps somewhat hasty in employing the terms " systematic disparagement," which I used merely to represent a series of depreciating criticisms from time to time on my several experimental researches, laying in the path of Dr. Riess's subsequent inquiries, and which certainly appeared systematically connected with each other. I fully acquit Dr. Riess, however, of any premeditated injustice toward me, and I am not too proud to withdraw this expression in a way courteous and, I trust, satisfactory to Dr. Riess. My memoir, as a scientific document, I willingly leave to the dispassionate judgement of disinterested persons, who will, I am sure, properly appreciate all I have advanced, and who will perhaps judge better than I can, whether Dr. Riess has really given any substantial reply to it or not.

I remain, Gentlemen,

Your obliged and faithful Servant,

W. Snow Harris.

6 Windsor Villas, Plymouth,
    8th July, 1856.

## XXI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

Dec. 13, 1855.—Colonel Sabine, R.A., Treas. and V.P., in the Chair.

THE following communication was read :—
 " On the Structure and Development of the *Cysticercus cellulosæ*, as found in the Pig." By George Rainey, Esq.

The *Cysticercus cellulosæ*, in its mature state, consists of two parts : one a small oval cyst, composed of a very thin membrane, rendered uneven on its external surface by minute rounded projections, and containing in its interior, granular matter, particles of oil, and a colourless fluid. This may be called its ventral portion. The other is folded inwards, occupying the centre of the cyst just described, but by pressure it may be made to protrude. This part is sometimes called the neck. Its length varies very much in different *Cysticerci*, depending upon their age. It is hollow, having strong membranous parietes, wrinkled transversely, and composed both of circular and longitudinal fibres. The cavity has no visible communication with that of the ventral portion. It contains a multitude of small oval laminated calcareous bodies, which, when acted upon by acids, effervesce briskly, and become partially dissolved, leaving only a small residue of animal matter. When the neck is protruded, the extremity farthest from the cyst is seen to present an enlargement, sometimes called the head, on the free surface of which there is a quadrangular area, occupied by four circular disks and a ring of hooklets. Each angle contains a disk, and the hooklets are placed in a circle around the centre of this space. The suctorial disks are traversed each by a passage taking rather a spiral course, and terminating in the cavity of the neck. The membrane composing a disk presents two orders of fibres, circular and radiating. The hooklets are generally twenty-six in number, thirteen long and as many short, arranged alternately a long and a short one. Each consists of a curved portion like a bird's claw, and a straight portion or handle ; and at the junction of these two parts there are tubercles, two in the short hooklets, and only one in the long ones. The hooklets are crossed by two zones of circular fibres. They are also connected by radiating fibres, which occupy the spaces between each adjacent pair, like the interosseous muscles situated between the metacarpal bones and phalanges. The hooklets are disposed like radii, with their points turned outwards and the extremities of their handles inwards, which, not meeting, circumscribe a circular space whose centre corresponds to that of the quadrangular area before mentioned. At this part there is no perforation answering to an oral orifice, but here the membrane is simply depressed so as to present a conical hollow. By pressure upon the neck, this membrane can be made to protrude in the form of a tongue-like process, to which the handles of all the hooklets are connected, so that when this

part in the living animal is made to move, the handles of the hook-lets will be drawn in with it, and their points carried from the entozoon, and thus made to penetrate the part to which it attaches itself.    These entozoa are chiefly found in the cellular intervals between the muscular fibres, contained in an adventitious cyst formed by the condensation of the surrounding tissues.    No more than one entozoon is ever met with in one cyst.

### *Development of the* Cysticercus cellulosæ.

The earliest appearance of the incipient stage of the *Cysticercus cellulosæ* is a fusiform collection of small cells and molecules in the substance of a primary muscular fasciculus, or immediately beneath its sarcolemma.    These cells, in this condition of the entozoon, have only an imperfect or partial covering; however, they soon become completely enclosed in a well-defined membrane which is at first homogeneous, but which afterwards sends out short, slender, projecting fibres, resembling short hairs or cilia.    These hair-like fibres, though resembling in some respects cilia, differ from them in being much less sharply defined and less pointed; however, for convenience sake, I shall speak of them as cilia. Their direction is remarkable.    At either extremity of the fusiform animal they are reflected backwards at a very acute angle, like the barbs of a feather, their direction being of course opposite at the two ends.    They become less and less inclined as they approach the middle of the body, where they stand out at right angles to the sur-face.    The apparatus of cilia-like processes above described is evi-dently designed to give to the entozoon, whilst in this stage of its existence, the power of penetrating between the ultimate muscular fibrillæ, and thus to enable it to force its way from the interior of a primary fasciculus into the spaces between the muscular fibres.    This will be the effect of the friction of the fibrillæ against the cilia, which will allow of motion in one direction only.    And as its two ends must move in opposite directions, the cilia will also serve to aid the entozoon in its development longitudinally.    That such is their office will be apparent on examining a sufficient number of speci-mens; in some of which the primary fasciculi will be seen to have been completely split up by these animals.    But the correctness of this inference is more strikingly proved by the influence which the size and arrangement of the primary bundles of muscular fibres have upon the form and dimensions of the entozoa.    Thus in the muscular parietes of the heart, where the primary fasciculi are smaller, and, from their frequent interlacing, shorter than in other parts, the cysticerci are, in this stage of their development, also very short and of a different form to those found in other muscles, composed of striped fibre, although in other respects perfectly similar; and, when completely formed, those taken from the heart cannot be distinguished from those formed in other muscles.    The cells which have been alluded to as forming the principal part of the *Cysticercus* thus far developed, and contained in the investment first described, are all of the same character, differing only in their form and size, according to

their age and situation. Those situated about the centre, and form-
ing the chief part of its bulk, are collected together into rounded
masses, giving to many of the animalcules an obscurely annulose
appearance. They are of an elliptical, or rather reniform figure.
This form, however, is not essential to these cells, but merely re-
sults from the circular shape of the masses into which they enter,
the convexity of each cell being a part of the outline of its
respective mass. These cells contain minute granules, or rather
molecules, which are variously disposed in different cells, so as to
present a variety of appearances, such as circular spaces, which
might be mistaken for nuclei, but which seem rather to be pro-
duced by a deficiency of the cell's contents at these parts, than by
any distinct nucleus. The mode of formation of these cells must be
examined in the growing parts of the animal, and for this purpose its
extreme ends are best adapted. When one of these ends is about
to have an addition made to its length, the investing membrane
at this part becomes at first very thin, and then disappears. A
clear space is next seen, having in some specimens the form of the
part which is about to be added to the extremity of the entozoon;
in others it has no defined limit. This space contains, in some
cases, nothing but extremely minute molecules, of different
shapes; in others, these molecules are mixed with granules of
various sizes, which have every appearance of having been pro-
duced by the coalescence of the molecules; and lastly, with these
molecules and granules, there are in other examples very distinct
globular cells, of a bright aspect, looking more like nuclei than
perfect cells; these soon become flattened oval, and ultimately take
the elliptical form before described. All the time these changes are
taking place in the molecules and cells, the membrane has been in
progress of formation, so that when the molecules have disappeared,
and their place has become occupied by perfect cells, the end of the
animal is completed. The cilia are soon afterwards added. The
lateral growth of these animals takes place in the same manner: the
first indication is a separation of the cilia, which, it must be observed,
are larger at the sides of an entozoon than at the extreme ends;
and then a thinning of the membrane supporting them; and, lastly,
the formation of globular cells, as before noticed. After the animals
have become of a considerable size, and forced their way from the
interior of the primary fasciculi into the cellular spaces between
the larger muscular fibres, they still continue to grow, especially
in breadth; but they lose their cilia, and gradually acquire those
parts which have been described as belonging to the neck. The
first evidence of this addition is the appearance of inversion of the
middle part of the cyst, forming a small hollow, the sides of which
look as if thrown into folds containing granular matter, and the
bottom presents a circular space in which are granular particles of
various forms and sizes, but those in the centre are darker than the
rest. It is from these particles that the suctorial disks, the hook-
lets, and the first of the laminated bodies are about to be formed,
but as yet none of these parts are recognizable. At a stage a little

more advanced, this apparent inversion of the cyst has increased, the neck has become longer, and the appearance of disks, hooklets, and laminated bodies is sufficiently distinct to be perfectly recognizable. The process of development is particularly apparent in the hooklets, and perhaps there is no other instance of the growth of an animal tissue which presents such facilities for the examination of the manner in which it is effected. First, because the part of the entozoon on which these organs are formed, is sufficiently transparent to admit of examination by the highest magnifying powers without any previous dissection. Secondly, because the material of which they are composed is so characteristic, and so dissimilar to the surrounding parts, that it can be detected in the minutest possible quantities. And, thirdly, as only a few of these hooklets are in progress of development at one time, and as these are in all stages of formation, every step in the progress of their growth can be traced from the merest molecule to a perfect hooklet. This is important in reference to the general theory of development, as it furnishes an example of the formation of a complete set of organs, on a plan more simple, and at variance with the cell-theory of Schwann and others. Before one of these hooklets takes on a recognizable form, it exists as a group of exceedingly refractive particles, all apparently of the same composition, and of a more or less globular form, but of very different sizes, some being so minute as scarcely to be visible by one-eighth of an inch lens, others being almost as large as the handle of a perfect hooklet, while the rest are of all dimensions between these extremes. The next condition of a hooklet is the apparent fusion or coalescence of some of these particles into the hooked part of the organ. Then the handle and tubercles are added, these having been previously formed by the fusion of the smaller particles, and these latter by the coalescence of the minutest and the minuter ones. Before the several parts are perfectly consolidated, their points of junction can be distinguished, and in other groups the fragments corresponding to those recently united can be recognized. Directly a hooklet is found, it is of its full dimension; and some of its parts are even larger and more clumsy-looking than in older hooklets. The substance of the particles entering into these organs, after they are once formed, undergoes no change in its microscopical characters, but is the same after as before their union. It is impossible to single out any one particle from the rest, which can be taken for the nucleus of a cell, or for what physiologists would call a nucleated cell; and thus there is nothing which indicates that these organs have been formed by transformation of previously existing cells, but, on the contrary, there is every appearance that their formation is due to the simple coalescence of homogeneous molecules.

Up to the present point, the facts which I have stated are so obvious, that their accuracy will, I think, not be questioned; also the interpretation of them is not only that which appears to me the most natural, but is almost self-evident. There remain,

however, some considerations of a more theoretical kind, though not of less importance. It will be asked, how the entozoon, in its earliest condition, such as I have described it, finds access to the interior of a primary fasciculus. Before attempting to answer this question, I must observe that my description commences from a condition of this entozoon so complete, that no one, on examining it in this state with the microscope, will deny its perfect similarity to those of the higher form. But there are other links in the chain which I must now consider, and which so far have been omitted only because I wished to keep that which is certain distinct from that which is probable. Before the cells and molecules already described accumulate in sufficient quantity to present the un-doubted character above mentioned, they are found aggregated in smaller groups, and even occurring individually in all the primary fasciculi of the diseased muscle; their quantity, and the size and form of these groups, present the greatest possible irregularity in the different fasciculi. In some the molecular deposit looks like an early stage of fatty degeneration, but it has characters very different; one is the shape of the molecules, which resemble in all respects those in the growing ends of an entozoon; and another is, their situation, which seems to be between the primary fibrillæ, tending to separate them longitudinally; however that may be, it is an abnormal condition, and always co-existent with the higher forms of the *Cysticercus*; and as the entozoon, as I have first described it, could not possibly have taken on that form all at once, these groups of molecules must therefore be looked upon as its antecedent stage, or as portions of *Cysticerci* in progress of development. But I also find in the specimens of muscle infested with these entozoa, many of the capillaries and smaller blood-vessels filled with organic molecules, which, so far as I am able to judge from the comparison of such extremely minute bodies, seem to resemble those molecules which are found in the primary fasciculi. The vessels filled with these molecules have their coats so thin as to be inappreciable, and some of the capillaries appear to be partially destroyed, and their molecular contents diffused among the sarcous elements. As this is an abnormal condition of the contents of these vessels, as well as of their coats, and, so far as my experience goes, is not found ex-cepting in conjunction with the earliest stages of the *Cysticerci*, I am inclined to believe that the molecules in question are the same as those in the primary fasciculi, and that it is by their coalescence in these fasciculi that the formation-cells of the *Cysticerci* are formed.

*Addendum*, Dec. 6.—After an entozoon has left the interior of a primary fasciculus, and arrived at the space between the muscular fibres, it loses its ciliated investment, and increases in breadth. Its margin now seems to be formed entirely by the convexities of the globular masses of cells of which its body appears to be made up, causing it to present a crenate form similar to that of the ven-tral portion of the perfect animalcule, with this difference only, that these cells are compressed. The next change which is visible

is the formation of folds, which become more perceptible as the animal increases in breadth, and which remain in the perfect entozoon so long as it is confined to a small space, but disappear when it gets to the space between the surface of a muscle and the fascia covering it. The unfolding in this last situation seems to be produced by the imbibition of fluid, and the consequent distension of the ventral part. These more advanced stages of the worm-form are best found in those specimens of diseased muscle in which the perfectly developed *Cysticerci* abound. Their number in proportion to that of the perfect animalcules varies considerably in different specimens.

I have always succeeded in finding some of those of the worm-form along with the perfectly developed ones; and in some cases there are as many of one kind as the other. After they have acquired a certain breadth—about one-twelfth, or the one-eighth of an inch,—the central part of the cyst appears to be drawn inwards, forming a hollow; at the bottom of which, the granular material is deposited from which the suckers, hooklets, and calcareous granules are formed, as above described.

Jan. 10, 1856.—Admiral Beechey, V.P., in the Chair.

The following communication was read:—

"On Insolinic Acid." By Augustus W. Hofmann, Ph.D., F.R.S.

In attempting to purify cuminic acid by boiling with chromic acid, I observed that this acid experienced, on the part of this reagent, a progressive alteration. By twenty-four hours' ebullition, cuminic acid is completely converted into an acid insoluble in alcohol and ether, for which I propose the provisional name of *insolinic acid*; purified by the ordinary processes, this body furnished on analysis the following relations:—

$$C_9 H_4 O_4;$$
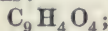
but the analysis of the salts demonstrates that this formula must be doubled, insolinic acid being a bibasic acid.

I have examined the following salts:—

|  |  |  |
|---|---|---|
| Insolinic acid .............. | $C_{18} H_8$ | $O_8$ |
| Silver salt ................. | $C_{18} (H_6 Ag_2)$ | $O_8$ |
| Copper salt............ .... | $C_{18} (H_6 Cu_2)$ | $O_8$ |
| Barium salt ............... | $C_{18} (H_6 Ba_2)$ | $O_8$ |
| Calcium salt (at $100°$ C.) ...... | $C_{18} (H_6 Ca_2)$ | $O_8$ |
| „     (at $133°$ C.) ...... | $C_{18} (H_6 Ca_2)$ | $O_8$ |
| Potassium salt (neutral) ...... | $C_{18} (H_6 K_2)$ | $O_8$ |
| „     (acid).......... | $C_{18} (H_7 K)$ | $O_8$ |
| Potassium-sodium salt ........ | $C_{18} (H_6 K Na) O_8$ | |

When considered by itself, insolinic acid has but slight claims on the attention of chemists; but when viewed in connexion with other groups of bodies, it acquires increased interest. Some years since, Gerhardt pointed out that to the homologous series of monobasic fatty acids $C_{n_2} H_{n_2} O_4$, the lowest terms of which are formic and acetic acids, there runs parallel a homologous series of bibasic acids, $C_{n_2} H_{n_2-2} O_8$, the simplest member of which is oxalic acid.

These two series of acids are connected by the closest ties, and very conclusive experiments have demonstrated that the members of the former may be easily converted into those of the latter; such is the case of the transformation of butyric into succinic acid, effected by M. Dessaignes under the influence of oxidizing agents.

The following table exhibits these two series of acids arranged according to their carbon :—

| | |
|---|---|
| Formic acid ......... | $C_2\ H_2\ O_4$ |
| Acetic acid............ | $C_4\ H_4\ O_4$ |
| Propionic acid ....... | $C_6\ H_6\ O_4$ |
| Butyric acid ......... | $C_8\ H_8\ O_4$ |
| Valeric acid ......... | $C_{10}\ H_{10}\ O_4$ |
| Caproic acid ......... | $C_{12}\ H_{12}\ O_4$ |
| Œnanthylic acid ..... | $C_{14}\ H_{14}\ O_4$ |
| Caprylic acid ......... | $C_{16}\ H_{16}\ O_4$ |
| Pelargonic acid ....... | $C_{18}\ H_{18}\ O_4$ |
| Rutic acid ........... | $C_{20}\ H_{20}\ O_4$ |
| Oxalic acid........... | $C_4\ H_2\ O_8$ |
| ——? ............ | $C_6\ H_4\ O_8$ |
| Succinic acid ......... | $C_8\ H_6\ O_8$ |
| Pyrotartaric acid ..... | $C_{10}\ H_8\ O_8$ |
| Adipic acid ......... | $C_{12}\ H_{10}\ O_8$ |
| Pimelic acid ......... | $C_{14}\ H_{12}\ O_8$ |
| Suberic acid ......... | $C_{16}\ H_{14}\ O_8$ |
| ——? ............ | $C_{18}\ H_{16}\ O_8$ |
| Sebacic acid ......... | $C_{20}\ H_{18}\ O_8$ |

The existence and the mode of formation of insolinic acid prove that to the series of monobasic aromatic acids, $C_{n_2}\ H_{n_2-8}\ O_4$, the lowest known term of which is benzoic acid, there corresponds likewise a series of bibasic acids, $C_{n_2}\ H_{n_2-8-2}\ O_8 = C_{n_2}\ H_{n_2-10}\ O_8$. Of this series few members are at present known, but the group of aromatic acids is itself very imperfect and limited. The two series comprise at present the following terms :—

| | |
|---|---|
| Benzoic acid ......... | $C_{14}\ H_6\ O_4$ |
| Toluylic acid ......... | $C_{16}\ H_8\ O_4$ |
| ——? ............ | $C_{18}\ H_{10}\ O_4$ |
| Cuminic acid ......... | $C_{20}\ H_{12}\ O_4$ |
| ——? ............ | $C_{14}\ H_4\ O_8$ |
| Terephthalic acid ...... | |
| Phthalic acid ......... | $\Big\}\ C_{16}\ H_6\ O_8$ |
| Insolinic acid ......... | $C_{18}\ H_8\ O_8$ |
| ——? ............ | $C_{20}\ H_{10}\ O_8$ |

If we take the carbon as the standard of comparison, it is evident that the bibasic insolinic acid corresponds to the monobasic acid, which stands between toluylic and cuminic acid. In addition to this unknown acid, toluylic acid only is represented in the series of bibasic acids. There are, in fact, two bodies which may be regarded as representatives of toluylic acid, namely, phthalic and terephthalic acids. Benzoic and cuminic acid are not yet represented.

Jan. 17.—Prof. William Allen Miller, M.D., V.P., in the Chair.

The following communication was read :—

"Anatomical and Physiological Observations on the Organ of Vision, for the purpose of ascertaining the alterations caused by section of the Optic Nerve." By Augustus Waller, M.D., F.R.S.

The author begins by referring to experimental researches already published by him on the effects produced on the nerves of living animals by section at different points in their course; and he briefly recapitulates the chief results of these experiments, as follows, viz.—
1. Section of a spinal nerve on the distal side of its ganglion is followed by degeneration of the part of the nerve beyond the section, whilst the part still connected with the ganglion retains its integrity.
2. Section of the posterior root of a spinal nerve induces no change in the trunk or branches of the nerve beyond the ganglion, nor in the portion of the root which remains connected with the ganglion, whereas the part of the root attached to the spinal cord, but disconnected with the ganglion, becomes disorganized. His conclusion from these experiments, which have been varied in different ways, is "that the spinal ganglion is the centre of the nutritive power of the nerve-fibres adherent to it." From other experiments, he concluded that the anterior spinal roots derive their nutritive influence from the spinal cord; and he has little doubt "that every nerve-fibre is in connexion with a nerve-cell, which exerts over it a peculiar influence which enables it to retain its structure," &c. He does not think it proved, however, that all ganglia exert an influence both in a central and peripheral direction like those of the posterior spinal roots, and indeed experiments have shown that certain ganglia, such as the superior cervical of the sympathetic, exert their sustaining power on the nerve-fibres connected with them, in one direction only.

The investigation of the effects of section on the optic nerve, which forms the subject of the present communication, promised to yield interesting results on several grounds, and especially as calculated to throw light on the relations of the fibres of the nerve to those of the opposite one in the optic chiasma or commissure, and on the question as to the manner in which they are connected with the brain.

After some observations on the structure of the optic nerve, the author describes his experimental procedure. To see the nerve whilst operating, and thus be assured that the section was effected at the desired point, he began by dislocating the eyeball forwards from its socket, while the animal was under the influence of ether. This operation he found could be effected in the rabbit, by simply pressing the eyelids widely apart, and was unattended with any serious disturbance of the functions of the organ. In dogs the operation is more difficult, and occasions more injurious effects. When the eye is dislocated, its axis is inclined downwards and inwards, and the optic nerve can be reached without difficulty; so that it may readily be cut at any desirable distance behind its place of entrance into the eyeball, and may even by moderate traction be broken off immediately before the chiasma.

After stating various observations which he made on the dislocated eye, the author gives an account of the effects which resulted from section of the optic nerve. The condition of the retina after the operation, was studied during the life of the animal by means of the ophthalmoscope; and, after death, its structure, as well as that of the optic nerve before and behind the place of section, the chiasma, the optic tracts, and connected part of the brain, was examined with the microscope.

The elements of the retina, as well as those of Jacob's membrane, were found unaltered four months after the time of section. The distal part of the optic nerve (that left in connexion with the eyeball), examined after the lapse of a month in one case, and of two months in another, was also discovered to be unaltered. On the other hand, the part of the nerve behind or on the central side of the section was invariably disorganized. The section was usually performed on the optic nerve of the right eye, and the disorganized fibres of its central segment could be traced back to the left optic tract, through the chiasma, where they obviously decussated with the sound fibres of the opposite nerve. The right optic tract had undergone no change; the fibres of the left tract were disorganized as far back as the quadrigeminate bodies, except those running along the posterior or inner border of the tract; which exception appears to the author to favour the opinion that fibres pass along the tracts and commissures from the quadrigeminate bodies of one side to those of the other side, without connecting themselves with the retina. On the other hand, the results of his experiments do not seem to him to countenance the notion of fibres running in the optic nerves from one retina to the other without connexion with the brain, nor the generally received doctrine that part of the fibres of the optic nerve are continuous with the optic tract of the same side; on the contrary, the whole fibres of the nerve would seem to undergo decussation.

The microscopic characters of the atrophied and disorganized nervous substance are described in the paper; they were found to differ somewhat in the part of the nerve before and that behind the chiasma, owing no doubt to the different structure of these parts in the sound state.

The changes produced in the geniculate and quadrigeminal bodies will be communicated in the succeeding part of the paper.

### GEOLOGICAL SOCIETY.

June 18, 1856.—*Special General Meeting.*—Sir C. Lyell, Vice-President, in the Chair.

It having been announced from the Chair that, in consequence of the lamented decease of Daniel Sharpe, Esq., the late President, the Meeting was called to elect a President and a Member of Council, the Meeting proceeded to ballot and unanimously elected Col. Portlock, R.E., to the Office of President, and Hugh Falconer, M.D., F.R.S., as a Member of Council.

*Ordinary Meeting.*—Col. Portlock, President, in the Chair.

The following communications were read:—

1. " Note on a Section of Mont Lacha near Mont Blanc." By Major S. Charters, F.G.S.

In a letter to Dr. Fitton, the author enclosed a section of the anthraciferous schist of M. Lacha (containing *Ammonites* and *Belemnites*), that he had taken during a tour five years ago, and which shows the strata dipping northward at an angle of 75°; whilst the lines of cleavage are perpendicular to the stratification. At first sight, says the author, the cleavage-planes might easily have been mistaken for stratification, but the strata are evident on careful observation, and do not dip towards or under Mont Blanc, but their dip is normal, supposing Mont Blanc to be the centre of upheaval.

2. " Notice of the late Eruption of Manna Loa, Owhyhee." By W. Miller, Esq., H.M. Consul, Sandwich Islands. (Forwarded from the Foreign Office by order of Lord Clarendon.)

In a letter dated January 9, 1856, Mr. Miller stated that the lava-stream originating in the eruption which broke out in August last, continued flowing to the date of his letter, at which time it appeared to have been arrested in the forest at a distance of about ten miles from Hilo. In a later communication, dated March 1, 1856, Mr. Miller describes the lava-stream as being sixty miles in length, and as having now reached within five or six miles of Hilo. There still intervenes about three miles' width of dense forest between it and the open ground towards Hilo, Byron's Bay. It has burnt its way through the forest at the rate of about one mile in two weeks.

3. " On the Geology of Varna and Neighbourhood, Bulgaria." By Capt. Spratt, R.N., F.G.S.

Capt. Spratt first noticed a series of whitish calcareous sandstones and marls, nearly 1000 feet thick, overlaid by reddish sands and marls. The former are of marine origin and of Eocene tertiary date; the latter are chiefly of freshwater origin. Near Varna the freshwater beds have been much denuded, and are not anywhere more than 200 feet thick. At Cape Aspro, fifteen miles south of Varna, both of the series—the grey and the red deposits—are seen disturbed, and dipping to the south, but unconformably, one series (the lower) having an angle of 30°, whilst the upper dips at 20°. At Cape Emineh, south of Cape Aspro, and forming the termination of the Balkan, these beds are still more disturbed and dip to the north. Capt. Spratt then described the geological appearances along the coast southward. At the Gulf of Bourgas and in the vicinity are igneous rocks, and deposits formed from their waste. Granite occurs on the southern point of the bay.

Returning to Varna, Capt. Spratt pointed out the localities of the fossils collected in the neighbourhood. The calcareous sandstones abound in casts of shells and in Oysters and Pectens immediately around Varna; and contain *Nummulites* in profusion at the upper part of the Lake near Allahdyn. In this last-named neighbourhood the uppermost strata, left by the denuding agencies that have affected

the district, are more durable than the underlying marls, &c., and have a thickness of about 20 or 30 feet. They consist of a stony mass of *Nummulites, Operculinæ,* and *Orbitoides,* with *Pectines, Terebratulæ,* and *Ostræa.* This harder portion of the superficial rock has become apparently weather-worn into vertical pillars, either isolated or still connected above by an horizontal layer of hard rock which has resisted the destructive action of the weather. Capt. Spratt observed that in some places in the vicinity the surface-rock was split by vertical cracks, so as to resemble an open pavement. These fissures, operated upon by atmospheric agencies, illustrate, in the author's opinion, the method in which the columnar fragments above alluded to, and other masses more or less spherical, remaining on the land, must have originated. The *Nummulites* contained in the disintegrating rock have not been destroyed, but remain intact, lying about in heaps around the remaining nodules of limestone.

Capt. Spratt referred to the possibility of this columnar state of the hard rock of the upper marine series having been brought about, during the period which intervened between the deposition of the marine series and that of the freshwater beds overlying the marine series in the neighbourhood, by means of water-action ; as it is possible that the columnar surface of the degraded eocene beds may have been covered up by the later deposits, and subsequently reexcavated. This opinion seems to be supported by the fact of columns occurring in a part of the Bay of Varna, at about 5 fathoms depth. But Capt. Spratt leans to the opinion that the columnar degradation is atmospheric, modern, and in actual progress.

Capt. Spratt then described the geology of the coast north of Varna. The Eocene deposits (yellowish limestone and sandy marls) occur as far nearly as Mangalia. The reddish freshwater sands and marls then come in, overlying, and form generally the steppes of the Dobrudja. Land shells occur in some of the upper beds of this district. The author then dwelt on the points of correspondence between the rocks composing the termination of the Balkan with those of the Crimea, and of the steppes of the Dobrudja with the northern part of the Crimea.

Capt. Spratt proceeded next to consider the age of the overlying red marls and sands ; and pointed out their resemblance to the freshwater deposits on the northern shore of the Sea of Marmora, on the Macedonian coast, the northern end of Eubœa, and the Locrian shore. In fact, almost all the Thracian peninsula is composed of freshwater deposits of brown and grey marls and sandstones, nearly horizontal and attaining about 500 feet of thickness, which appear to be contemporaneous with the upper pleiocene freshwater deposits on the western side of the Archipelago, in Eubœa and Macedonia, and in Rhodes, &c. on the south.

The author concluded with a notice of a post-tertiary or recent marine deposit on the coast of the Dardanelles at a height of about 15 or 20 feet above the present sea-level.

4. "Notes on the Geology of Trinidad," By H. G. Bowen, Esq., F.G.S.

The northern district of the Island of Trinidad, with the islands

between it and the mainland, is composed of flagstones, slates, and schists, with quartz-veins and some dark-coloured intercalated lime-stone. These rocks are all apparently unfossiliferous ; the slates often abound with iron, and some of the quartz-veins are slightly auriferous. Stalactitic caves occur in the limestone of the Island of Gaspar Grande, and at Las Cuevas and Arouca. Alluvial beds of clay and gravel are extensive in this district, and are sometimes 60 feet thick. At Lateen Bay, in Chicachicare Island, a patch of aluminous clay-slate occurs, with seams of crystalline limestone. The soil of this northern district is fertile on the limestone, and barren on the slates. The slate-rocks appear to be the same as those of Venezuela, which overlie quartz-rock at Upata ; and rounded boulders of quartz-rock occur in the flagstones.

In the south of the Island of Trinidad, red sandstone abounds, often ferruginous, and associated with clays which are often either bituminous or pyritous, and contain lignite and impressions of dico-tyledonous leaves. In the Erin district the clay-beds have been sometimes indurated and jasperized by heat. They afford also small chalybeate and sulphuretted hydrogen springs, and in the blue-clay formation are found hillocks throwing up mud and water, and ponds covered by a film of mineral tar. The mud-volcanoes throw up saline water and greyish mud, in a cold state, with iron-pyrites and water-worn pebbles of blue limestone like that of the northern part of the Island, and sometimes of sandstone. They do not appear to be connected with the sea ; and are most active at the close of the rainy season. At Moruga small hills of granular limestone occur. The succession of deposits in this southern part of Trinidad appears to be—beginning from below—1. Sandstone, variegated sands, lignitiferous clays (sometimes jasperized), and the Moruga lime-stone ; 2. Blue and brown clays, with bitumen; comprising the pitch-lakes, salt and alum springs, &c. ; 3. Modern marine sand formation, from 50 to 100 feet thick ; and alluvial deposits, seldom more than 30 feet thick.

The eastern coast of Trinidad appears to consist of the red sand-stones and bituminous clays as far north as Matura, beyond which the clay-slates set in.

The western coast of the island, south of Port of Spain, which is built of the slate-rocks and limestone, exhibits only modern alluvial deposits, sometimes calcareous, frequently ferruginous, and resting towards the south on the red sandstone of the southern district.

5. " On the fossils found in the Chalk-flints and Greensand of Aberdeenshire." By J. W. Salter, Esq., F.G.S., and W. Baily, Esq.

A notice of the occurrence of chalk-flints and greensand in Aber-deenshire has been published by W. Ferguson, Esq., F.G.S. in the Proceed. Glasgow Phil. Soc. vol. iii. p. 33, and the Phil. Mag. 1850. p. 430, and some of the facts had been previously noticed ; but no lists of the fossils had been given. This communication showed the presence of characteristic Upper Greensand fossils in the low ground at Moreseat: *Thetis minor, Arca carinata, Pinna tetragona,* and *Galerites castanea.* The *Lima elegans* of Nilsson is a new fossil for Britain, and is found with the ordinary *Inocerami* and *Echinites*

of the Chalk in the rolled flints which form terraces round the hills in Aberdeenshire. The probable continuity, therefore, of these beds with those of the south of Sweden, where the same order of succession prevails, is inferred ; the extension of the Upper Greensand so far north is a point of much interest. The Antrim beds are probably Lower Greensand only : at least their age is doubtful.

6. " On the Correlation of the Middle Eocene Tertiaries of England, France, and Belgium." By J. Prestwich, Esq., F.R.S., Treas. G.S.

In a former paper the author had shown the correlation of the strata beneath the Bracklesham series in England, the Calcaire grossier and Lits Coquilliers in France, and the Upper Ypresian system in Belgium, and which he had proposed to designate as the " London Tertiary Group," from the circumstance of these strata attaining the largest and most distinct development in the English area. In the present paper Mr. Prestwich entered into an account of the structures of the deposits next above. In France this is the Calcaire grossier, which the French geologists have divided into four stages : —1. a lower one of green sands, with few fossils ; 2. a middle one, of a calcareous freestone, abounding in marine organic remains (Grignon, Courtagnon, and other celebrated localities being in beds of this zone) ; 3. an upper division, of harder and more flaggy calcareous rock, rich in *Miliolites* and *Cerithium*, mixed with a few freshwater shells and the remains of plants and land animals ; and 4. a series of white and light green marls, apparently of freshwater origin. Each division attains at places a thickness of 30 to 40 feet, but the lower ones are thickest in the centre and west of the Paris basin ; whilst the upper ones, on the contrary, are thickest to the eastward. The total thickness of the deposit, therefore, rarely at any one place exceeds 100 feet, whilst the Upper Bracklesham series, with which it corresponds, is more than 500 feet thick. This difference the author attributed to a more rapid subsidence of the English area than of the French at that geological period. This, he showed, was accompanied by more marine conditions prevailing all through the English deposit, and by the continuance throughout of the same green sands which in France were confined to the lower division. That the whole series was, however, synchronous with the Calcaire grossier he considered proved by the circumstance, that, although the freshwater beds which existed in France did not extend to this country, yet the organic remains of some of the beds of the Bracklesham series gave evidence of one upper division higher than the beds with the *Venericardia planicosta* and *Cerithium giganteum* of Bracklesham, for at the latter place the proportion of shells ranging up into the overlying Barton series was 30 to 100, whereas in some beds recently discovered by Mr. F. Edwards at Bramshaw, and apparently at the top of the Bracklesham series, the proportion is 46 to 100. The middle beds of the Bracklesham series show the closest affinity with the Middle Calcaire grossier, although there are only 140 species in common. The lowest division of this series is more fossiliferous in England than in France, showing a closer relation (43 to 100)

with the underlying beds than does the mass of the Calcaire grossier, in which the proportion is as 28 to 100. The total number of Molluscs in the Calcaire grossier of the Oise is 651, and in the Bracklesham series of Hampshire 368.

Above this zone is the series of the Grès Moyen in France and Barton clays in England. Owing to the number of Calcaire grossier fossils which had been found at Barton, these beds had been considered synchronous with the Calcaire grossier, a view which the author himself had formerly adopted with reserve. Seeing, however, that the Bracklesham series probably represented all the divisions of the Calcaire grossier, and that the distinction between the Bracklesham and Barton series was of equal value to that between the Calcaire grossier and the Sables moyens, the author now correlated the Barton clays with the Sables moyens, as suggested by M. Graves, M. Dumont, Sir Charles Lyell, and M. Hébert. He, however, alluded to the difficulty of doing this upon the evidence of any small number of organic remains, or even of a few species considered characteristic in one area ; and he showed that in the Barton clay itself, although there were many Grès moyen species (63), still there were a greater number of Calcaire grossier species (69). In the same way in the Laekenian system of Belgium, which overlaid the Bruxellian system (the equivalent of the Calcaire grossier), there are forty-five Calcaire grossier and Bracklesham sand species, and only forty-four Barton and Grès moyen species. But Mr. Prestwich showed that, taking the per-centage of species which range from the lower to the higher series, each area offered nearly an equal amount of distinction, as out of 100 species of the lower series there are in England 30, in France 35, and in Belgium 32, which range upwards.

Mr. Prestwich mentioned that M. Graves had recognized several well-known Barton species, such as the *Voluta depauperata, V. athleta, Oliva Branderi, Conus scabriusculus,* &c., in the Sables moyens of the Oise. The total known number of the Sables moyens species is 377, and of the Barton clays 252.

These series the author proposed to term the " Paris Tertiary Group " (its lower part), as the several members of it were more complete in France than in England, and contained a richer and better-preserved fauna. This Paris group forms the great Nummulitic zone. Hitherto none of these Rhizopoda have been found in the London group. The author concluded with some general observations on the extent of the ancient seas and the position of the dry land, and took occasion to observe on the fact, that, although the several deposits in each country were so rich in organic remains, yet only a small proportion of them had hitherto been identified as common to the several areas. Nevertheless the same genera prevailed, and the relative number of species of each genus was generally tolerably well maintained. He hoped, therefore, that Palæontologists would, in cases where there was now good reason to believe the strata to be synchronous, inquire into the extent of variation which the same species might undergo in areas where the sea had presented

such different conditions of depth, mineral composition of sea-bottom, &c. A certain number of peculiar species must necessarily result from such different conditions, but the author considered it probable that the same causes would lead to the existence of such marked varieties as might, viewing each area separately and independently, cause some varieties to assume the permanence and importance of specific differences. Until the exact synchronism of any deposit is established, the Palæontologist cannot always fully take these causes into consideration, and many admirable monographs on Tertiary fossils have necessarily been founded, in a great measure, upon the differences actually apparent, and persistent in the several areas.

Mr. Prestwich stated that it was his intention to continue this inquiry at a future period, and to examine into the correlation of the curious and interesting freshwater and fluviatile series overlying the Barton clay on the Hampshire coast and in the Isle of Wight.

## XXII. *Intelligence and Miscellaneous Articles.*

### ON THE HEAT ABSORBED IN CHEMICAL DECOMPOSITIONS.

#### *To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,                                              Manchester, July 4, 1856.

IN your Number for this month, p. 74, Dr. Woods mis-states the results I arrived at fifteen years ago, and endeavours to support his own claims as an original discoverer by setting up one portion of my papers in contradiction to another. He says that when I caused the current to pass through an electrolyte, I did not find the same law to exist as when it passed through a metallic conductor. On the contrary, he will find in my paper (Phil. Mag. Oct. 1841, pp. 270–274) the account of seven experiments, by which it was proved, that in an electrolytic cell the heat is evolved proportionally to the resistance to conduction and the square of the current,—the same law which I had shown to exist in the case of a metallic wire and in that of the cell of a battery. Also at p. 275, he will see a general rule expressed as follows:—
" When any voltaic arrangement, whether simple or compound, passes a current of electricity through any substance, whether an electrolyte or not, the total voltaic heat which is generated in any time is proportional to the number of atoms which are electrolyzed in each cell of the circuit, multiplied by the virtual intensity of the battery." A foot-note to the word ' virtual ' explains its meaning by saying, that " If a decomposing cell be in the circuit, the *virtual* intensity of the battery is reduced in proportion to its resistance to electrolyzation."
I think that the meaning of the above proposition is sufficiently clear, but will nevertheless illustrate it by two examples. Suppose, first, we take a voltaic battery of 20 iron-zinc pairs, and connect its terminals by a metallic wire. After a certain interval of time, we find 100 atoms or chemical equivalents of zinc dissolved in each cell of

the battery. Then the entire amount of heat evolved will be expressed by $100 \times 20 = 2000$. Suppose, secondly, we employ the same battery to decompose water; the virtual intensity of the entire battery circuit will then be $20 - 3\frac{1}{3} = 16\frac{2}{3}$ (see p. 272 of the above-named memoir); and when 100 atoms of zinc in each cell of the battery have been dissolved, or what comes to the same thing, when 100 atoms of hydrogen have been liberated in the decomposing cell, the heat evolved by the entire circuit will be expressed by $100 \times 16\frac{2}{3} = 1666$. The difference between 2000 and 1666, or 334, represents of course the heat taken up in effecting the separation of 100 atoms of hydrogen from 100 atoms of oxygen, and liberating them in the gaseous state.

In p. 276 it will be seen that I deduced the heat of combustion of hydrogen from the quantity of heat due to the intensity of battery required to electrolyze water. In other words, I found the heat of combustion of hydrogen by observing the heat lost in the voltaic circuit when water was decomposed.

My subsequent papers confirm and extend the facts arrived at in 1841, and indicate the effects of secondary actions in the battery, the laws which regulate the evolution of heat when positive metals, such as zinc, are employed as electrodes evolving hydrogen, the mechanical value of chemical changes, &c. It is in giving a more defined expression to the law of the evolution of heat by combustion that Dr. Woods erroneously supposes I have contradicted myself. The passage he refers to (Phil. Mag. Dec. 1843, pp. 442, 443) simply states " more explicitly, that it is not precisely the attraction of affinity, but rather the mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and consequently the quantity of heat evolved." This modification appeared to me necessary, inasmuch as, according to the mechanical theory of heat, the product of the attraction of particles into the distance they have to traverse in order to combine, rather than the attraction itself, is to be considered as the source of heat.

If, therefore, my paper on the heat disengaged in chemical combinations, sent to the French Academy in 1846, and published in the Philosophical Magazine, June 1852, were to go for nothing, my claim to priority would be completely established; but I maintain, along with the most eminent scientific authorities, that a paper dates from the time that its receipt has been certified by a scientific body. My opinion is confirmed by the practice of the Royal Society and other learned bodies, who cause the date on which a paper comes to hand, as well as that on which it is read, to be duly certified. I may add, that Faraday has dated his ' Experimental Researches ' from the time they were received by the Secretary of the Royal Society. My own paper was not only duly acknowledged in the *Comptes Rendus* for February 9, 1846, but was referred to a Commission. It still remains in the possession of the Academy.

I have the honour to remain, Gentlemen,

Yours very respectfully,

James P. Joule.

## CLEAVAGE OF COMPRESSED WHITE LEAD.

7 St. Mary's Road, Canonbury,
July 21, 1856.

MY DEAR TYNDALL,

In an account of your lecture " On the Cleavage of Crystals and Slate Rocks " (Phil. Mag. vol. xii. p. 46), you have noticed my experience in the consolidation of precipitated white lead, and have correctly stated the unfavourable results of my first attempts with conical moulds. It may be of interest to the readers of the Philosophical Magazine to learn that the difficulty was entirely overcome by substituting cylindrical moulds. By their employment the lateral spreading of the mass was prevented, and cakes of white lead were produced which broke with a conchoidal fracture, and which consequently could readily be ground to a granular powder.

I am,

Yours very truly,

WARREN DE LA RUE.

## ON THE BEHAVIOUR OF IODIDE OF SILVER TOWARDS AMMONIA.
### BY DR. A. VOGEL, JUN.

The yellow precipitate furnished by iodide of potassium with nitrate of silver, is not dissolved by ammonia, but acquires a paler colour therein. The author has found that this change of colour is owing to a combination of the iodide of silver with ammonia.— Buchner's *Neues Repert.* vol. v. p. 53.

### ELECTROLYTICAL INVESTIGATIONS. BY M. MAGNUS.

It is well known that saline solutions do not follow the law of equivalent decomposition by the galvanic current, as established by Faraday, for Daniell found that they are decomposed in such a manner that, besides 1 equiv. of acid and base, 1 equiv. of hydrogen and oxygen are separated. Thus when a voltameter with dilute sulphuric acid is interposed in the circuit together with the saline solution, equal quantities of gas are evolved from both the saline solution and the voltameter; but besides this, the salt is decomposed, and 1 equiv. of free acid is separated at the positive electrode, and 1 equiv. of free base at the negative. The decomposition in the saline solution is therefore twice as great as that in the voltameter.

To explain this remarkable phænomenon, or bring it into agreement with Faraday's law, Daniell found himself compelled to regard sulphate of soda, not as consisting of base and acid in accordance with the general view, but as composed of sodium with a compound of 1 atom of sulphur and 4 atoms of oxygen. He gave the name of oxysulphion to this compound, and was of opinion that all oxysalts have a similar composition; so that sulphate of copper must be regarded as consisting of copper and oxysulphion, nitrate of potash,

of potassium and oxynitrion, and so forth, an opinion which he has subsequently endeavoured to establish in a more comprehensive inquiry published in conjunction with Professor Miller.

Although this view agrees with that first put forward by Sir Humphry Davy regarding the composition of salts, and though much may be said in its favour upon purely chemical grounds, as has been done by Berzelius in the third volume of his *Lehrbuch*, yet many more reasons may be brought against it, as stated by Berzelius in the same place; amongst which one of the principal is, that neither oxysulphion nor any of the analogous compounds has yet been prepared.

Almost all those who have occupied themselves with the decomposition of salts by the galvanic current since Daniell and Miller, have found themselves obliged to admit the correctness of Daniell's explanation. This is the case especially with Professor Buff, in his memoir upon the Law of Electrolysis; De la Rive in the recently published second part of his *Traité d'électricité*; and E. Becquerel in his memoir *Des lois qui président à la décomposition électro-chimique des corps*. Hittorff alone, in his investigation of the travelling of the *ions*, starts from a different explanation, which, however, does not appear to get rid of the difficulties of the case.

The author has, in the first place, repeated Daniell's experiments. He obtained the same general results; but a full equiv. of acid and base was not always separated for 1 equiv. of oxygen, the former amounting only to between 60 and 80 per cent. of the equiv. of the oxygen. He then passed to the explanation of this apparently double decomposition, and shows that for this purpose Daniell's supposition is unnecessary. He is much more inclined to conclude from his experiments, that to separate a simple body from a compound the same force is always required, whether it is combined with only one simple body to form a binary, or with several bodies to form a saline compound.

Starting from this point, it is not difficult to prove that Faraday's law of equivalent decomposition is applicable not only to binary, but also to saline and all other inorganic compounds.

Daniell, as appears from his reply to the objections raised against him by Dr. Hare, found himself compelled to adopt his theory, especially because he could in no other way explain how the same current which bore the metal to the negative electrode should convey the oxygen and acid, and consequently two bodies in the opposite direction. But a transference, in the sense in which Daniell appears to have understood it, does not take place; and it may be shown, at least in certain cases, that for 1 equiv. of metal or hydrogen which is set free at the negative electrode, 1 equiv. of acid and 1 equiv. of oxygen must be set free at the positive electrode.

In order to mark these cases more decidedly, the author next occupied himself with the conditions under which the separation of a substance takes place from an electrolyte in which several separable substances are present. He has found that the separation depends,—

1. On the density of the current;

2. On the proportions in which the different substances exist in the fluid;

3. On the nature of the electrodes;

4. On the greater or less facility with which one or the other substance can be carried from stratum to stratum within the fluid, as well as on the obstacles which stand in the way of this transmission, either in the shape of porous walls or in any other form.—*Bericht. der Akad. der Wiss. zu Berlin,* 1856, p. 158.

## METEOROLOGICAL OBSERVATIONS FOR JUNE 1856.

*Chiswick.*—June 1. Hazy: cloudy. 2, 3. Very fine. 4. Very fine: cloudy: lightning at night. 5—7. Very fine. 8. Dull and cloudy. 9, 10, Very fine. 11. Cloudless. 12. Very fine: rain at night. 13. Rain. 14. Showery and boisterous. 15. Very fine: cloudy: clear and fine. 16. Very fine. 17. Showery. 18. Very fine: cloudy: rain. 19. Rain. 20. Showery. 21. Very fine: cloudy: rain. 22. Cloudy and fine. 23. Overcast. 24. Very fine: uniformly overcast. 25. Very fine. 26. Sultry. 27. Cloudless and hot. 28. Uniformly overcast: sultry: cold at night: range of temperature 45°. 29, 30. Clear and dry air.

| | |
|---|---|
| Mean temperature of the month | 58°·65 |
| Mean temperature of June 1855 | 57 ·98 |
| Mean temperature of June for the last thirty years | 60 ·31 |
| Average amount of rain in June | 1·880 inch. |

*Boston.*—June 1. Cloudy: rain A.M. and P.M. 2—6. Fine. 7. Cloudy. 8—11. Fine. 12. Cloudy: rain P.M. 13, 14. Cloudy: rain A.M. and P.M. 15. Cloudy. 16. Fine. 17. Rain A.M. 18. Cloudy. 19. Cloudy: rain A.M. 20. Cloudy. 21. Fine: rain P.M. 22. Cloudy: rain A.M. and P.M. 23, 24. Cloudy. 25. Fine. 26. Cloudy: thermometer 86° P.M. 27. Fine: thunder P.M. 28. Cloudy. 29. Fine. 30. Rain A.M. and P.M.

*Sandwick Manse, Orkney.*—June 1. Cloudy A.M.: drizzle P.M. 2. Drizzle A.M.: bright P.M. 3. Cloudy A.M. and P.M. 4. Cloudy A.M.: drops P.M. 5. Bright A.M.: cloudy P.M. 6. Cloudy A.M. and P.M. 7. Rain A.M.: bright P.M. 8. Clear A.M. and P.M. 9. Cloudy A.M.: drops, clear P.M. 10. Clear A.M.: showers P.M. 11. Rain A.M. and P.M. 12, 13. Bright A.M.: clear, fine P.M. 14. Cloudy A.M.: sleet-showers P.M. 15. Clear A.M.: cloudy P.M. 16. Showers A.M.: cloudy P.M. 17. Showers A.M. and P.M. 18. Bright A.M.: showers P.M. 19. Rain A.M.: bright P.M. 20. Cloudy A.M.: bright P.M. 21. Rain A.M.: bright P.M. 22. Bright A.M. and P.M. 23. Clear A.M.: cloudy P.M. 24. Fog A.M.: drizzle P.M. 25. Hazy A.M.: damp P.M. 26. Drops A.M.: rain, clear P.M. 27. Bright A.M.: damp P.M. 28. Damp A.M.: showers P.M. 29, 30. Clear A.M.: cloudy P.M.

| | |
|---|---|
| Mean temperature of June for previous twenty-nine years | 52°·76 |
| Mean temperature of this month | 51 ·47 |
| Mean temperature of June 1855 | 52 ·23 |
| Average quantity of rain in June for previous sixteen years | 2·24 inches. |

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at* CHISWICK, *near London; by Mr. Veall, at* BOSTON; *and by the Rev. C. Clouston, at Sandwick Manse,* ORKNEY.

| Days of Month. 1856. June. | Barometer. | | | | | Thermometer. | | | | | Wind. | | | Rain. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Chiswick. | | Boston. | Orkney, Sandwick. | | Chiswick. | | Boston. | Orkney, Sandwick. | | Chiswick. | Boston. | Orkney, Sandwick. | Chiswick. | Boston. | Orkney, Sandwick. |
| | Max. | Min. | 8¾ a.m. | 9¼ a.m. | 8½ p.m. | Max. | Min. | 8¾ a.m. | 9¼ a.m. | 8½ p.m. | 1 p.m. | | | | | |
| 1. | 29·738 | 29·690 | 29·25 | 29·88 | 29·79 | 56 | 33 | 49·5 | 53 | 53 | w. | n. | e. | | ·12 | ·06 |
| 2. ● | 29·891 | 29·798 | 29·49 | 29·62 | 29·60 | 73 | 36 | 55 | 49 | 49 | sw. | w. | nw. | | ·24 | ·10 |
| 3. | 30·017 | 29·957 | 29·53 | 29·64 | 29·75 | 77 | 39 | 60 | 52 | 51 | sw. | w. | calm | | | ·02 |
| 4. | 30·108 | 30·046 | 29·54 | 29·77 | 29·86 | 75 | 45 | 59 | 55½ | 48½ | w. | w. | w. | | | |
| 5. | 30·143 | 30·103 | 29·61 | 30·02 | 30·13 | 63 | 33 | 57 | 51 | 49½ | ne. | nw. | wnw. | | | ·12 |
| 6. | 30·253 | 30·189 | 29·75 | 30·16 | 30·10 | 66 | 46 | 57 | 55½ | 52 | n. | nw. | sse. | | | |
| 7. | 30·282 | 30·218 | 29·78 | 29·88 | 29·87 | 73 | 45 | 63 | 56½ | 48 | sw. | w. | wnw. | | | ·35 |
| 8. | 30·203 | 30·178 | 29·63 | 29·98 | 29·94 | 72 | 52 | 64 | 53½ | 51 | sw. | wsw. | s. | | | |
| 9. | 30·128 | 30·062 | 29·54 | 29·82 | 29·81 | 75 | 49 | 65 | 55 | 48½ | sw. | wsw. | ssw. | | | ·01 |
| 10. D | 30·083 | 30·064 | 29·58 | 29·72 | 29·76 | 76 | 37 | 59 | 52 | 50 | ne. | w. | wnw. | | ·09 | ·09 |
| 11. | 30·066 | 30·004 | 29·56 | 29·48 | 29·44 | 76 | 39 | 65 | 53½ | 50 | s. | s. | w. | | ·18 | ·43 |
| 12. | 29·920 | 29·854 | 29·37 | 29·54 | 29·61 | 66 | 55 | 67 | 53 | 53 | sw. | ssw. | sse. | ·12 | | ·04 |
| 13. | 29·777 | 29·525 | 29·32 | 29·66 | 29·67 | 64 | 54 | 63 | 53½ | 47 | sw. | sw. | n. | ·24 | ·03 | |
| 14. | 29·779 | 29·568 | 29·00 | 29·55 | 29·53 | 65 | 37 | 57 | 51½ | 52 | sw. | sw. | ssw. | ·04 | | |
| 15. | 30·102 | 29·909 | 29·45 | 29·70 | 29·67 | 75 | 35 | 58 | 49 | 52 | nw. | sw. | s. | | | ·11 |
| 16. | 30·143 | 30·022 | 29·64 | 29·62 | 29·51 | 74 | 44 | 57 | 54½ | 48 | sw. | sw. | w. | | | ·01 |
| 17. | 29·921 | 29·850 | 29·34 | 29·34 | 29·54 | 74 | 42 | 58 | 52 | 47½ | w. | s. | ese. | ·04 | | ·09 |
| 18. ○ | 29·905 | 29·785 | 29·44 | 29·69 | 29·67 | 65 | 45 | 62·5 | 51 | 47 | w. | w. | ne. | ·12 | ·04 | ·07 |
| 19. | 29·623 | 29·493 | 29·18 | 29·67 | 29·74 | 62 | 49 | 63·5 | 48½ | 48 | s. | s. | sse. | ·08 | | ·03 |
| 20. | 29·941 | 29·533 | 29·06 | 29·78 | 29·78 | 67 | 40 | 57 | 52 | 49 | sw. | w. | calm | ·16 | ·02 | ·10 |
| 21. | 30·053 | 29·982 | 29·57 | 29·62 | 29·62 | 68 | 54 | 57·5 | 54 | 47½ | sw. | w. | nne. | ·08 | ·07 | ·02 |
| 22. | 29·968 | 29·938 | 29·40 | 29·75 | 29·97 | 68 | 50 | 65 | 54 | 49 | nw. | n. | sse. | | | ·26 |
| 23. | 30·225 | 30·093 | 29·66 | 30·09 | 29·98 | 69 | 42 | 55·5 | 54 | 53 | ne. | w. | w. | | | |
| 24. | 30·190 | 30·148 | 29·68 | 29·78 | 29·81 | 68 | 55 | 62·5 | 58 | 53 | w. | w. | w. | | | ·06 |
| 25. ☾ | 30·178 | 30·159 | 29·63 | 29·80 | 29·89 | 82 | 50 | 67 | 55½ | 57 | w. | nw. | sw. | | ·13 | ·22 |
| 26. | 30·211 | 30·162 | 29·66 | 29·98 | 29·90 | 88 | 56 | 72·5 | 53½ | 57 | nw. | nw. | w. | | | |
| 27. | 30·144 | 30·030 | 29·55 | 29·86 | 29·95 | 86 | 57 | 76 | 51 | 51 | sw. | nw. | w. | | | ·09 |
| 28. | 30·162 | 30·038 | 29·54 | 29·89 | 29·93 | 82 | 37 | 67 | 52 | 48½ | n. | nw. | wnw. | | | ·03 |
| 29. | 30·256 | 30·189 | 29·76 | 30·10 | 30·10 | 77 | 42 | 56 | 51 | 48 | sw. | sw. | e. | | ·08 | ·05 |
| 30. | 30·114 | 29·997 | 29·55 | 30·15 | 30·11 | 80 | 49 | 58·5 | 51⅜ | 49 | nw. | w. | n. | | | |
| Mean. | 30·050 | 29·952 | 29·50 | 29·785 | 29·801 | 72·40 | 44·90 | 61·1 | 52·93 | 50·00 | | | | 0·88 | 1·00 | 2·36 |

XXIII. *Further Researches on the Polarity of the Diamagnetic Force. By* JOHN TYNDALL, *F.R.S., Membre de la Société Hollandaise des Sciences; Foreign Member of the Physical Society of Berlin, and Professor of Natural Philosophy in the Royal Institution*[*].

## *Introduction.*

A YEAR ago I placed before the Royal Society the results of an investigation "On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet[†]." The simultaneous exhibition of attraction and repulsion in the case of magnetized iron is the fact on which the idea of the polarity of this substance is founded; and it resulted from the investigation referred to, that a corresponding duality of action was manifested by bismuth. In those experiments the bismuth was the moveable object upon which fixed magnets were caused to act, and from the deflection of the bismuth its polarity was inferred. But, inasmuch as the action is reciprocal, we ought also to obtain evidence of diamagnetic polarity by reversing the conditions of experiment; by making the magnet the moveable object, and inferring from its deflection the polarity of the mass which produces the deflection. This experiment would be complementary to those described in the communication referred to, and existing circumstances invested the experiment with a great degree of interest and importance.

In fact an experiment similar to that here indicated was made by Professor W. Weber, previous to my investigation, and the

*Phil. Mag.* S. 4. Vol. 12. No. 78. *Sept.* 1856.          M

result was such as to satisfy its author of the reverse polarity of diamagnetic bodies. I will not here enter into a minute description of the instrument and mode of experiment by which this result was obtained ; for the instrument made use of in the present inquiry being simply a refinement of that made use of by M. Weber, its explanation will embrace the explanation of his apparatus. For the general comprehension of the criticisms to which M. Weber's results have been subjected, it is necessary, however, to remark, that in his experiments, a bismuth bar, within a vertical spiral of copper wire, through which an electric current was transmitted, was caused to act upon a steel magnet freely suspended without the spiral. When the two ends of the bar of bismuth were permitted to act successively upon the sus pended magnet, a motion of the latter was observed, which indicated that the bismuth bar was polar, and that its polarity was the reverse of that of iron.

Notwithstanding the acknowledged eminence of M. Weber as an experimenter, this result failed to produce general conviction. Mr. Faraday, in his paper "On the polar or other condition of diamagnetic bodies*," had shown that results quite similar to those obtained by M. Weber, in his first investigation with bismuth, were obtained in a greatly exalted degree, with gold, silver and copper ; the effect being one of induction, and not one due to diamagnetic polarity. He by no means asserted that his results had the same origin as those obtained by M. Weber ; but as the latter philosopher had made no mention of the source of error which Mr. Faraday's experiments rendered manifest, it was natural to suppose that it had been overlooked, and the observed action attributed to a wrong cause. In an article published in his ' Massbestimmungen ' in 1852, M. Weber, however, with reference to this point, writes as follows :—" I will remark that the article transferred from the Reports of the Society of Sciences of Saxony to Poggendorff's *Annalen* was only a preliminary notice of my investigation, the special discussion of which was reserved for a subsequent communication. It will be sufficient to state here, that in the experiments referred to I sought to eliminate the inductive action by suitable combinations ; but it is certainly far better to set aside this action altogether, as has been done in the experiments described in the present memoir."

One conviction grew and strengthened throughout these discussions—this, namely, that in experiments on diamagnetic polarity great caution is required to separate the pure effects of diamagnetism from those of ordinary induction. With refer-

* Experimental Researches, 2640, Philosophical Transactions, 1850, p. 171.

ence to even the most recent experiments of M. Weber—those to which he has referred at the conclusion of the above citation —it is strongly urged that there is no assurance that the separation referred to has been effected. In those experiments, as already stated, a cylinder of bismuth was suspended within a vertical helix of covered copper wire, and the action of the cylinder upon a magnet suspended opposite to the centre or neutral point of the helix was observed. To increase the action, the position of the cylinder was changed at each termination of the minute swing of the magnet, the amplitude of the oscillations being thus increased, and the effect rendered more sensible to the eye. Now, it is urged, there is every reason to believe that in these motions of a metallic mass within an excited helix induced currents will be developed, which, acting upon the magnet, will produce the motions observed. The failure indeed to demonstrate the existence of diamagnetic polarity by other means has, in the case of some investigators, converted this belief into a certainty.

Among the number whom M. Weber's experiments have failed to convince, M. Matteucci occupies a prominent place. With reference to the question before us this philosopher writes as follows\*:—"In reading the description of the experiments of M. Weber, we are struck on beholding the effects produced by moving the bismuth when there is no current in the spiral. Although the direction of oscillation in this latter case is opposed to that observed when the spiral is active, still the fact excites doubts as to the truth of the conclusions which have been drawn from these experiments†. *To deduce rigorously the demonstration of diamagnetic polarity, it would be necessary to substitute for the bismuth, masses formed of insulated fragments of the metal‡, to vary the dimensions of the cylinder, and above all, to compare the effects thus obtained with those which would probably be obtained with cylinders of copper and silver in a state of purity.*

"We are obliged to make the same remarks on another series of experiments which this physicist has made to obtain anew, by the effects of induction, the proof of diamagnetic polarity.

---

\* *Cours Spécial sur l'Induction*, p. 206.

† It is not my place to account for the effect here referred to. I may however remark, that there appears to be no difficulty in referring it to the ordinary action of a diamagnetic body upon a magnet. It is the result which Brugmann published upwards of half a century ago; the peculiar form of this result in one of the series of experiments quoted by M. Weber must, I think, be regarded as purely accidental.—J. T.

‡ Also in page 204:—"Il fallait donc, pour prouver si l'influence d'un corps diamagnétique produit sur un aimant une variation de sens contraire à celle développée dans le fer doux, opérer avec ce corps *privé de conductibilité*."

It is astonishing, that after having sought to neutralize the
development of induced currents in the moving cylinders of
bismuth, by means of a very ingenious disposition of the spiral
—it is astonishing, I repeat, that no attempt was made to prove
by preliminary essays with metals possessing a higher conducti-
bility than bismuth, that the same end could be obtained.    I
cannot leave you ignorant that the doubts which I have ventured
to advance against the experiments of M. Weber are supported
by the negative result which I have obtained in endeavouring to
excite diamagnetic polarity in bismuth by the discharge of the
Leyden jar."

It will be seen in the following pages that the conditions laid
down by M. Matteucci for the rigorous demonstration of diamag-
netic polarity are more than fulfilled.

The conclusions of M. Weber find a still more strenuous
opponent in his countryman Professor v. Feilitzsch, who has
repeated Weber's experiments, obtained his results, but who
denies the validity of his inferences.    M. v. Feilitzsch argues,
that in the experiments referred to it is impossible to shut out
ordinary induction, and for the rigorous proof of diamagnetic
polarity proposes the following conditions*.    "To render the
experiment free from the action of induced currents two ways
are open.    The currents can be so guided that they shall mutually
neutralize each other's action upon the magnet, or the induced
currents can be *completely got rid of* by using, instead of a dia-
magnetic *conductor*, a diamagnetic *insulator*."    To test the ques-
tion, M. v. Feilitzsch resorted to the latter method : instead of
cylinders of bismuth he made use of cylinders of wax, and also
of a prism of heavy glass, but in neither case was he able to
detect the slightest action upon the magnet.    "However the
motions of the prism might be varied, it was not possible either
to cause the motionless magnet to oscillate, or to bring the
magnet from a state of oscillation to one of rest."    M. v. Fei-
litzsch pushes his experiments further, and finds that when the
bismuth is *motionless* within its spiral, the position of the mag-
net is just the same as when the bismuth is entirely withdrawn ;
hence his final conclusion, that the deflection of the magnet in
Weber's experiments is due to induced currents, which are ex-
cited in the bismuth by its mechanical motion up and down
within the spiral.

These divergent opinions upon a question of such vital bearing
upon the general theory of magnetic phænomena, naturally ex-
cited in me the desire to make myself acquainted with the exact
value of M. Weber's experiments.    The most direct way of
accomplishing this I consider to be, to operate with an instru-

* Poggendorff's *Annalen*, xcii. 377.

ment similar to that made use of by Weber himself; I therefore resolved to write to the constructor of his apparatus, but previous to doing so the thought occurred to me of writing to M. Weber, to inquire whether his further reflections on the subject had suggested to him any desirable modification of his first instrument. In reply to my question he undertook to devise for me an apparatus, surpassing in delicacy any hitherto made use of. The design of M. Weber was ably carried out by M. Leyser of Leipzig; and with the instrument thus placed in my possession, I have been able, not only to verify the experiments of M. Weber, but to satisfy the severest conditions proposed by those who saw in the results of these experiments the effects of ordinary induction.

## Description of Apparatus.

A sketch of the instrument made use of in the present investigation is given in fig. 2. BO, B'O' is the outline of a rectangular box, the front of which is removed so as to show the apparatus within. The back of the box is prolonged, and terminates in two semicircular projections, which have apertures at H and H'. Stout bolts of brass, which have been made fast in solid masonry, pass through these apertures, and the instrument, being secured to the bolts by screws and washers, is supported in a vertical position, being free from all disturbance save such as affects the foundations of the Royal Institution. All the arrangements presented to the eye in fig. 2 are made fast to the back of the box, but are unconnected with the front, so as to permit of the removal of the latter. WW' are two boxwood wheels with grooved peripheries, which permit of motion being transferred from one wheel to the other by means of a string *ss'*. Attached to this string are two cylinders, *mn, op*, of the body to be examined : in some cases the cylinders are perforated longitudinally, the string passes through the perforation, and the cylinders are supported by knots on the string. HE, H'E' are two helices of copper wire overspun with silk, and wound round two brass reels, the upper ends of which protrude from H to G, and from H' to G'. The internal diameter of each helix is 0·8 of an inch, and its external diameter about 1·3 inch; the length from H to E is 19 inches, and the centres of the helices are 4 inches apart; the diameters of the wheels WW' being also 4 inches. The cross bar GG' is of brass, and through its centre passes the screw R, from which depends a number of silk fibres which support an astatic arrangement of two magnets, the front one of which, SN, is shown in the figure. An enlarged section of the instrument through the system of magnets is shown in fig. 4. The magnets

are connected by a brass cross-piece, in which is the point of suspension P; and the position of the helices is shown to be between the magnets. It will be seen that the astatic system is a horizontal one, and not vertical, as in the ordinary galvanometer. The black circle in front of the magnet SN, fig. 2, is a mirror, which is shown in section at M, fig. 4; to balance the weight of this mirror, and adjust the magnets in a horizontal posi-
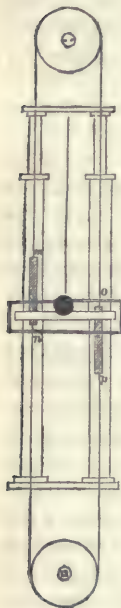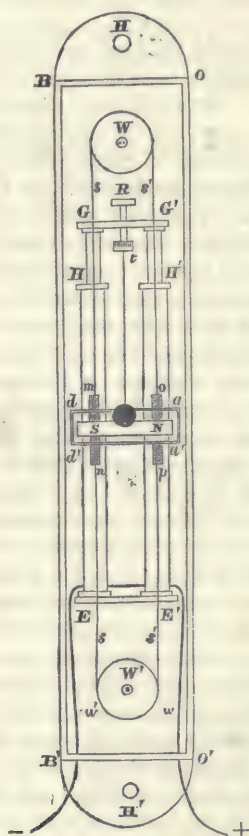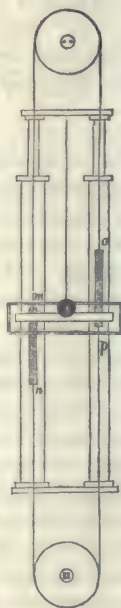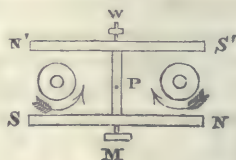


Fig. 1.      Fig. 2.      Fig. 3.

Fig. 4.

tion, a brass washer, W, is caused to move along a screw, until a point is attained at which its weight has brought both the magnets into the same horizontal plane. There is also another adjustment, which permits of the magnets being brought closer together or separated more widely asunder. The motions of the compound

magnet are observed by means of a distant scale and telescope, according to the method applied to the magnetometer of Gauss. The rectangle $da$, $d'a'$, fig. 2, is the outline of a copper damper, which, owing to the currents induced in it by the motion of the magnet, soon brings the latter to rest, and thus expedites experiment.

It is well known that one end of a magnet attracts, while the other end repels the same pole of a magnetic needle; and that between both there is a neutral point which neither attracts nor repels. The same is the case with the helices HE, H'E'; so that when a current is sent through them, if the astatic magnet be exactly opposite the neutral point, it is unaffected by the helices. This is scarcely attainable in practice; a slight residual action remains which draws the magnets against the helices; but this is very easily neutralized by disposing an external portion of the circuit so as to act upon the magnets in a direction opposed to that of the residual action. Here then we have a pair of spirals which, when excited, do not act upon the magnets, and which therefore permit us to examine the pure action of any body capable of magnetic excitement placed within them. In the experiments to be described, it was always arranged that the current flowed in opposite directions through the two spirals; so that if the bodies within them were polar, the two upper ends of these bodies should be poles of opposite names, and consequently the two lower ends opposed also. Supposing now our two cylinders to occupy the central position indicated in fig. 2: even if the cylinders became polar through the action of the surrounding current, the magnets, being opposite to the neutral points of the cylinders, would experience no action from the latter. But suppose the wheel W' to be so turned that the two cylinders are brought into the position shown in fig. 1, the upper end $o$ of $op$ and the lower end $n$ of $mn$ will act simultaneously upon the suspended magnets. For the sake of illustration, let us suppose the ends $o$ and $n$ to be both north poles, and that the section, fig. 4, is taken when the bars are in the position shown in fig. 1. The right-hand pole $o$ will attract S' and repel N, which attraction and repulsion sum themselves together to produce a deflection of the system of magnets. On the other hand, the left-hand pole $n$, being also north, will attract S and repel N', which two effects also sum themselves to produce a deflection in the same direction as the former two. Hence, not only is the action of terrestrial magnetism annulled by this arrangement, but the moving force due to the reciprocal action of the magnets and the bodies within the helices is increased fourfold. By turning the wheel in the other direction, we bring the cylinders into the position shown in

fig. 3, and thus may study the action of the ends *m* and *p* upon the magnets.

By means of the screw R the magnets can be raised or lowered; and at the end, *t*, of the screw is a small torsion circle which can be turned independently of the screw; by means of the latter the suspending fibre can be twisted or untwisted without altering the level of the magnets.

The front of the box is attached by means of brass hasps, and opposite to the mirror M a small plate of glass is introduced, through which the mirror is observed; the magnets within the box being thus effectually protected from the disturbances of the external air.   A small handle to turn the wheel W′ accompanied the instrument from its maker; but in the experiments, I used, instead of it, a key attached to the end of a rod 10 feet long; with this rod in my right hand, and the telescope and scale before me, the experiments were completely under my own control.   Finally, the course of the current through the helices was as follows:—Proceeding from the platinum pole of the battery it entered the box along the wire *w*, fig. 2, which passed through the bottom of the latter; thence through the helix to H′, returning to E′; thence to the second helix, returning to E, from which it passed along the wire *w′* to the zinc pole of the battery. A commutator was introduced in the circuit, so that the direction of the current thus indicated could be reversed at pleasure.

### *Experiments.—Deportment of Diamagnetic Bodies.*

A pair of cylinders of chemically pure bismuth, 3 inches long and 0·7 of an inch in diameter, accompanied the instrument from Germany.   These were first tested, commencing with a battery of one cell of Grove.   Matters being as sketched in fig 2, when the current circulated in the helices and the magnet had come to rest, the cross wire of the telescope cut the number 482 on the scale.   Turning the wheel W′ so as to bring the cylinders into the position fig. 1, the magnet moved promptly, and after some oscillations took up a new position of equilibrium; the cross wire of the telescope then cut the figure 468 on the scale. Reversing the motion so as to place the cylinders again central, the former position 482 was assumed; and on turning further in the same direction, so as to place the cylinders as in fig. 3, the position of equilibrium of the magnet was at the number 493. Hence by bringing the two ends *n* and *o* to bear upon the astatic magnet, the motion was from greater to smaller numbers, the position of rest being then fourteen divisions less than when the bars were central.   By bringing the ends *m* and *p* to bear upon the magnet, the motion was from smaller to greater numbers,

the position of rest being eleven divisions more than when the bars were central.

As the positions here referred to will be the subject of frequent reference, for the sake of convenience I will call the position of the cylinders sketched in fig. 1, Position 1; that sketched in fig. 2, Position 2; and that sketched in fig. 3, Position 3.   The results which we have just described, tabulated with reference to these terms, would then stand thus :—

### I.

Bismuth cylinders.

Length 3 inches.          Diameter 0·7.

Position 1. 468          Position 2. 482          Position 3. 493

In changing therefore from position 1 to position 3, a deflection corresponding to twenty-five divisions of the scale was produced.

Wishing to place myself beyond the possibility of illusion as regards the fact of deflection, I repeated the experiment with successive batteries of two, three and four cells.   The following are the results :—

### II.

|            | 2 cells. | 3 cells. | 4 cells. |
| --- | --- | --- | --- |
| Position 1. | 450 | 439 | 425 |
| Position 2. | 462 | 450 | 437 |
| Position 3. | 473 | 462 | 448 |

In all the cases cited we observe the same result.   From position 2 to position 1 the motion is from larger to smaller numbers; while from position 2 to position 3 the motion is from smaller to larger numbers.

It may at first sight appear strange that the amount of the deflection did not increase with the battery power; the reason, in part, is that the magnet, when the current circulated, was held in a position free from the spirals, by forces emanating partly from the latter and partly from a portion of the external circuit. When the current increased, the magnetization of the bismuth increased also, but so did the force which held the magnets in their position of equilibrium.   To remove them from this position, a greater amount of force was necessary than when only the residual action of a feeble current held them there.   This fact, coupled with the circumstance that less heat was developed, and less disturbance caused by air currents, when a feeble battery was used, induced me for some time to experiment with a battery of two cells.   Subsequent experience however enabled me to change this for five cells with advantage.

Notwithstanding the improbability of the argument, still it may be urged that these experiments do not prove beyond a

doubt that the bismuth cylinders produce the motion of the magnets in virtue of their excitement by the voltaic current; for it is not certain that these cylinders would not produce the same motion wholly independent of the current. Something of this kind has occurred to M. Leyser * in his experiments, and why not here?

In answer to this, I reply, that if the case be as here suggested, the motion of the magnet will not be changed when the current surrounding the bismuth cylinders flows in the opposite direction. Here is the experiment.

### III.

Position 1. 764        Position 2. 742        Position 3. 704

We observe here that in passing from position 2 to position 1 the motion is from smaller to larger numbers; while in passing from position 2 to position 3 the motion is from larger to smaller numbers. This is the opposite result to that obtained when the current flowed in the opposite direction; and it proves that the polarity of the bismuth cylinders depends upon the direction of the current, changing as the latter changes. It was pleasant to observe the prompt and steady march of the magnet as the cylinders were shifted in the helices. When the magnets, operated on by the bars of bismuth, were moving in any direction, by bringing the two opposite ends of the bismuth bars into action, the motion could be promptly checked; the magnets could be brought to rest, or their movement converted into one in the opposite direction.

I may add to the above a series of results obtained some days subsequently in the presence of Professors Faraday, De la Rive and Marcet.

### IV.

#### Bismuth cylinders.

Position 1. 670        Position 2. 650        Position 3. 630

The difference between positions 1 and 3 amounts here to forty divisions of the scale; subsequent experience enabled me to make it still greater.

It was found by experiment, that when the motion was from lower to higher numbers it denoted that the poles NN', fig. 4, were repelled from the spirals, and the poles SS' attracted towards them. When, on the contrary, the motion was from larger to smaller numbers, it indicated that the poles NN' were attracted and the poles SS' repelled. In the position fig. 1, therefore, of Tables III. and IV. the poles NN' were repelled by the ends *no* of the bismuth cylinders, and the poles SS' attracted; while in

* Scientific Memoirs, New Series, vol. i. page 184.

the position fig. 3, the poles NN′ were attracted by the ends MP, and the poles SS′ repelled; the ends *n* and *o*, therefore, acted as two north poles, while the ends *m* and *p* acted as two south poles. Now the direction of the current in the experiments recorded in the two tables referred to was that shown by the arrows in fig. 4. Standing in front of the instrument, the direction in the adjacent face of the spiral H′E′ was from right to left, while it was from left to right in HE. From this we may infer that the polarity of the bismuth cylinders was the reverse of that which would be excited in cylinders of iron under the same circumstances. The assertion however shall be transferred from the domain of deduction to that of fact before we conclude.

Let us now urge against these experiments all that ever has been urged by the opponents of diamagnetic polarity. The bismuth cylinders are metallic conductors, and in moving them through the spirals induced currents more or less powerful may be excited in these conductors. The motion observed may not, after all, be due to diamagnetic polarity, but to the currents thus excited. I reply, that in all cases the number set down marks the *permanent* position of rest of the magnets. Were the action due to induced currents, these currents, being momentary, could only impart a *shock* to the magnets, which, on the disappearance of the currents, would return to their original position. But the deflection is permanent, and is therefore due to an enduring cause. In his paper on " Supposed Diamagnetic Polarity," Mr. Faraday rightly observes,—" If the polarity exists, it must be in the particles, and for the time permanent, and therefore distinguishable from the momentary polarity of the mass due to induced temporary currents, and it must also be distinguishable from ordinary magnetic polarity by its contrary direction." These are the precise characteristics of the force made manifest by the experiments now under consideration.

Further, the strength of induced currents depends on the conducting power of the mass in which they are formed. Expressing the conducting power of bismuth by the number 1·8, that of copper would be expressed by 73·6*, the conductivity of the latter being therefore forty times that of the former. Hence the demand made by the opponents of diamagnetic polarity, to have the experiments repeated with cylinders of copper; for if the effect be due to induced currents, they will show themselves in copper in a greatly increased degree. The following is the result of a series of experiments made with two copper cylinders, of the same dimensions as the bismuth ones already described.

* Philosophical Magazine, Series 4, vol. vii. p. 37.

### V.

### Cylinders of Copper.

Position 1. 754        Position 2. 754        Position 3. 755

Now if the effects obtained with bismuth were due to induced currents, we should have the same effects forty times multiplied in the case of copper, in place of which we have scarcely any sensible effect at all.

Bismuth is the only substance which has hitherto produced an action in experiments of this nature; another illustration, however, is furnished by the metal antimony, which possesses a greater conductive power, but a less diamagnetic power than bismuth. The following results were obtained with this substance:—

### VI.

### Cylinders of Antimony.

| Length 3 inches. | | Diameter 0·7. |
|---|---|---|
| | Current direct* . | Current reversed †. |
| Position 1. | 693 | 244 |
| Position 2. | 688 | 252 |
| Position 3. | 683 | 261 |

On comparing these numbers with those already obtained with bismuth, we observe that for like positions the actions of both metals are alike in direction. We further observe that the results are determined, not by the relative conductive powers of the two metals, but by their relative diamagnetic powers. If the former were the determining cause, we should have greater deflections than with the bismuth, which is not the case; if the latter, we should have less deflections, which is the case.

The third and severest condition proposed by those who object to the experiments of M. Weber is to substitute insulators for conductors. I call this condition severe for the following reasons:—according to the experiments of Faraday‡, when bismuth and sulphur are submitted to the same magnetic force, the repulsion of the former being expressed by the number 1968, that of the latter will be expressed by 118. Hence an action which, with the means hitherto used, was difficult of detection in the case of bismuth, must wholly escape observation in the case of sulphur, the intensity of whose excitement is nearly twenty times less. The same remarks apply, in a great measure, to all other insulators.

But the admirable apparatus made use of in this investigation has enabled me to satisfy this condition also. To Mr. Faraday

* As in III. and IV.        † As in I. and II.
‡ Phil. Mag. March 1853, p. 222.

I am indebted for the loan of two prisms of the self-same heavy glass with which he made the discovery of diamagnetism. The bismuth cylinders were withdrawn from the helices and the prisms of glass put in their places. It was now necessary to have a perfectly steady magnet, the expected result being so small as to be readily masked by, or confounded with, a motion arising from some extraneous disturbance. The feeble warmth developed in the helices by a current from two cells I found able to create air currents of sufficient power to defeat all attempts to obtain the pure action of the prisms. To break up these currents I stuffed all unfilled spaces of the box with old newspapers, and found the expedient to answer perfectly. With a fresh battery, which delivered a constant stream throughout the duration of an experiment, the magnet was admirably steady*, and under these favourable conditions the following results were obtained :—

## VII.

### Prisms of Heavy Glass.

| Length 3 inches. | Width 0·6. | Depth 0·5. |
|---|---|---|
| Current direct. | Current direct. | Current direct. |
| Position 1. 664 | Position 2. 662 | Position 3. 660 |

Thus in passing from position 1 to 3, or *vice versâ*, a permanent deflection corresponding to four divisions of the scale was produced. By raising or lowering the respective prisms at the proper moments the amplitude of the oscillations could be considerably augmented, and when at a maximum, could be speedily extinguished by reversing the motions of the prisms. In six different series of experiments made with this substance the same invariable result was obtained. It will be observed that the deflections are in all cases in the same direction as those produced by bismuth under the same circumstances.

The following results were afterwards obtained with the same prisms in the presence of M. De la Rive; the current was "direct."

## VIII.

| Position 1. 652 | Position 2. 650 | Position 3. 648 |
|---|---|---|

On the negative result arrived at with this substance, it will be remembered that M. v. Feilitzsch bases one of his arguments against the conclusions of M. Weber.

Calcareous spar was next submitted to experiment. Two cylinders of the transparent crystal were prepared and examined in the manner already described. The results are as follows :—

* It was necessary however to select a portion of the day when Albemarle Street was free from cabs and carriages, as the shaking of the entire building, by the rolling of these vehicles, rendered the magnets unsteady.

## IX.

### Cylinders of Calcareous Spar.

Length 3 inches.                    Diameter 0·7.

Current direct.              Current direct.              Current direct.
Position 1. 699·5        Position 2. 698·5        Position 3. 697·5

Here, as in the other cases, the deflection was permanent, and could be augmented by the suitable raising or lowering of the respective cylinders. The action is small, but perfectly certain. The magnet was steady and moved promptly and invariably in the directions indicated by the numbers. It will be observed that the deflections are the same in kind as those produced by bismuth.

The intrusion of other employments compelled me to postpone the continuation of these experiments for several weeks. On taking up the subject again, my first care was to assure myself that the instrument retained its sensibility. Since the experiments last recorded it had been transported over several hundred miles of railway, and hence the possibility of a disturbance of its power. The following experiments, while they corroborate the former ones, show that the instrument retained its power and delicacy unimpaired :—

## X.

### Bismuth cylinders.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 612 | 264 |
| Position 2. | 572 | 230 |
| Position 3. | 526 | 200 |

The deflections, it will be observed, are the same in kind as before; but by improved manipulation the effect is augmented. In passing from position 1 to 3 we have here a deflection amounting in one case to 64, and in the other to 86 divisions of the scale.

To Mr. Noble I am indebted for two cylinders of pure statuary marble; the examination of these gave the following results :—

## XI.

### Cylinders of Statuary Marble.

Length 4 inches.                    Diameter 0·7.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 601 | 215 |
| Position 2. | 598 | 218 |
| Position 3. | 596 | 220 |

Here, in passing from position 1 to 3, we have a permanent

deflection corresponding to five divisions of the scale. As in all other cases, the impulsion of the magnet might be augmented by changing the position of the cylinders at the limit of each swing. The deflections are the same in kind as those produced by bismuth, which ought to be the case, as marble is diamagnetic.

An upright iron stove influenced by the earth's magnetism becomes a magnet with its bottom a north and its top a south pole. Doubtless, though in an immensely feebler degree, every erect marble statue is a true diamagnet, with its head a north pole and its feet a south pole. The same is certainly true of man as he stands upon the earth's surface, for all the tissues of the human body are diamagnetic.

A pair of cylinders of phosphorus enclosed in thin glass tubes were next examined.

## XII.

### Cylinders of Phosphorus.

Length 3·5 inches. Diameter 0·63.

|  | Current direct. | | Current reversed. |
|---|---|---|---|
|  | Series I. | Series II. | |
| Position 1. | 620 | 670 | 224 |
| Position 2. | 618 | 668 | 226 |
| Position 3. | 616 | 666 | 228 |

The change of the bars from position 1 to 3 is in this case accompanied by permanent deflection corresponding to four divisions of the scale. The deflection indicates the polarity of diamagnetic bodies. The magnet was remarkably steady during these experiments, and the consequent clearness and sharpness of the result pleasant to observe.

## XIII.

### Cylinders of Sulphur.

Length 6 inches. Diameter 0·7.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 658·5 | 222 |
| Position 2. | 657 | 223·5 |
| Position 3. | 655·5 | 225·5 |

## XIV.

### Cylinders of Nitre.

Length 3·5 inches. Diameter 0·7.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 648·5 | 263 |
| Position 3. | 647 | 265 |

Finally, as regards solid diamagnetics, a series of experiments was made with wax; this also being one of the substances whose negative deportment is urged by M. v. Feilitzsch against M. Weber.

### XV.

### Cylinders of Wax.

Length 4 inches.                    Diameter 0·7.

|            | Current direct. | Current reversed. |
|------------|-----------------|-------------------|
| Position 1. | 624·5          | 240               |
| Position 3. | 623            | 241               |

The action is very small, but it is nevertheless perfectly certain. The argument founded on the negative deportment of this substance must therefore give way. When we consider the feebleness of the action with so delicate a means of examination, the failure of M. v. Feilitzsch to obtain the effect, with an instrument constructed by himself, will not excite surprise.

Thus in the case of seven insulating bodies the existence of diamagnetic polarity has been proved; the list might be augmented without difficulty; but sufficient, I trust, has been done to remove the scruples of those who saw in M. Weber's results an action produced by induced currents.

A portion of the subject hitherto untouched, but one of great interest, has reference to the polar condition of liquid bodies while under magnetic influence. The first liquid examined was distilled water; it was enclosed in thin glass tubes, corked at the ends, and by means of a loop passing round the cork the tubes were attached to the string passing round the wheels WW'. Previous to use, the corks were carefully cleansed, so that any impurity contracted in cutting, or by contact with ferruginous matters, was completely removed. The following are the results obtained with this liquid :—

### XVI.

### Cylinders of Distilled Water.

Length 4 inches.                    Diameter 0·65.

|            | Current direct. | Current reversed. |
|------------|-----------------|-------------------|
| Position 1. | 605            | 246               |
| Position 2. | 603            | 248               |
| Position 3. | 601            | 250               |

The experiment was many times repeated, but always with the same result; indeed the polarity of the liquid mass is as safely established as that of iron. Pure water is diamagnetic, and the deflections produced by it are the same as those of all the other diamagnetic bodies submitted to examination.

From the position which it occupies in Mr. Faraday's list\*, I had also some hopes of proving the polarity of sulphide of carbon. The following results were obtained :—

### XVII.

Cylinders of Bisulphide of Carbon.
Length 4 inches.          Diameter 0·65.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 631 | 210 |
| Position 2. | 629 | 213 |
| Position 3. | 626 | 216 |

As in the case of distilled water, we observe a deflection in one direction when the current is "direct" and in the other when it is "reversed," the action in the first case, in passing from position 1 to 3, amounting to five, and in the latter case to six divisions of the scale. The polarity exhibited is that of diamagnetic bodies.

### *Deportment of Magnetic Bodies.*

Thus far we have confined our examination to diamagnetic substances; turn we now to the deportment of magnetic bodies when submitted to the same conditions of experiment. Here we must select the substances suitable for examination, for all are not so. Cylinders of iron, for example, of the same size as our diamagnetic cylinders, would, through the intensity of their action, quite derange the apparatus; so that we are obliged to have recourse to bodies of smaller size or of feebler magnetic capacity. Besides, the remarks of writers on this subject render it of importance to examine whether bodies through which the magnetic constituents are very sparingly distributed present a veritable polarity the same as that exhibited by iron itself.

Slate rock usually contains from eight to ten per cent. of oxide of iron, and a fragment of the substance presented to the single pole of an electro-magnet is attracted by the pole. A cylinder of slate from the Penrhyn quarries near Bangor was first examined. It was not found necessary to increase the effect by using two cylinders, and the single one used was suspended in the right-hand helix H′E′. The deportment of the substance was as follows :—

### XVIII.

Cylinder of Penrhyn Slate.
Length 4 inches.          Diameter 0·7.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 620 | 280 |
| Position 2. | 647 | 240 |
| Position 3. | 667 | 198 |

\* Phil. Mag. March 1853, p. 222.

Comparing these deflections with those obtained with diamag-
netic bodies, we see that they are in the opposite direction.
With the direct current a change from position 1 to 3 is fol-
lowed, in the case of diamagnetic bodies, by a motion from
higher to lower numbers; while in the present instance the
motion is from lower numbers to higher. In the former case
the north poles of the astatic magnet are attracted, in the latter
they are repelled. We also see that a *direct* current acting on
diamagnetic bodies produces the same deflection as a *reverse* cur-
rent on magnetic ones. Thus, as we promised at a former
page, the opposite polarities of diamagnetic and magnetic bodies
are transferred from the region of deduction to that of fact.

### XIX.

Cylinder of Caermarthen Slate.

Length 4 inches.                    Diameter 0·7.

|              | Current direct. | Current reversed. |
|--------------|-----------------|-------------------|
| Position 1.  | 664             | 300               |
| Position 2.  | 690             | 235               |
| Position 3.  | 720             | 185               |

The deflections in this case are also indicative of magnetic po-
larity.

These two cylinders were so taken from the rock that the axis of
each lay in the plane of cleavage. The following experiments,
made with a cylinder of the same size, show the capability of a
rock of this structure to be magnetized across the planes of
cleavage.

### XX.

Cylinder of Slate: axis of cylinder perpendicular to cleavage.

|              | Current direct. | Current reversed. |
|--------------|-----------------|-------------------|
| Position 1.  | 655             | 240               |
| Position 2.  | 678             | 205               |
| Position 3.  | 695             | 192               |

Chloride of iron was next examined: the substance, in powder,
was enclosed in a single glass tube, which was attached to the
string passing round the wheels WW' of the instrument.

### XXI.

Cylinder of Chloride of Iron.

Length 3·8 inches.                    Diameter 0·5.

|              | Current direct. | Current reversed. |
|--------------|-----------------|-------------------|
| Position 1.  | 185             | 990               |
| Position 2.  | —               | 230               |
| Position 3.  | 990             | 185               |

The deflection here indicates ordinary magnetic polarity. The action was very powerful. When swiftly moving in any direction, a change in the position of the cylinder instantly checked the magnet in its course, brought it to rest, or drove it forcibly in the opposite direction. The numbers 185 and 990 mark indeed the utmost limit between which it was possible for the magnet to move; here it rested against the helices.

Two glass tubes were filled with red oxide of iron and examined. The action of the poles of these cylinders upon the magnets was so strong, as to efface, by the velocity imparted to the magnets, all distinct impression of the numbers on the scale. By changing the position of the tubes within the helices, the magnets could be driven violently through the field of view, or could be held rigidly against the respective helices. As in all other cases, the centre of the cylinders were neutral points, and the two ends of each were poles of opposite qualities. The polarity was of course the same as that of iron.

A small quantity of iron filings was kneaded thoroughly in wax, and a cylinder formed from the mass. Its deportment was also very violent, and its polarity was of course just as clear and pronounced as that of a solid cylinder of iron could possibly be.

Sulphate of iron was next examined: the crystallized substance was enclosed in two glass tubes and tested in the usual manner.

## XXII.

### Cylinders of Sulphate of Iron.

Length 4·5 inches.          Diameter 0·7.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 510 | 510 |
| Position 2. | 600 | 370 |
| Position 3. | 700 | 220 |

The red ferroprussiate of potassa is a magnetic salt; with this substance the following results were obtained:—

## XXIII.

### Cylinders of red Ferroprussiate of Potassa.

Length 4·5 inches.          Diameter 0·65.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 610 | 250 |
| Position 2. | 630 | 220 |
| Position 3. | 655 | 197 |

In this case also the crystallized salt was enclosed in glass tubes.

Two glass tubes were next filled with carbonate of iron in the state of powder: the following are the results:—

## XXIV.

### Cylinders of Carbonate of Iron.
Length 4 inches.                    Diameter 0·5.

| Current direct. | Current direct. | Current direct. |
|---|---|---|
| Position 1. 185 | Position 2. 620 | Position 3. 740 |

In all these cases the deflections show that the bodies are polar after the manner of iron.

As the complement of the experiments made with diamagnetic liquids, we now pass on to the examination of the polarity of magnetic liquids. A concentrated solution of sulphate of iron was enclosed in two glass tubes and submitted to examination.

## XXV.

### Sulphate of Iron solution in tubes.
Length 4 inches.                    Diameter 0·65.

| Current direct. | Current direct. | Current direct. |
|---|---|---|
| Position 1. 548 | Position 2. 600 | Position 3. 648 |

A solution of muriate of nickel, examined in the same manner, gave the following results:—

## XXVI.

### Muriate of Nickel solution in tubes.
Length 3·6 inches.                  Diameter 0·65.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 605 | 224 |
| Position 2. | 632 | 200 |
| Position 3. | 650 | 185 |

A solution of muriate of cobalt yielded as follows:—

## XXVII.

### Muriate of Cobalt solution in tubes.
Length 3·6 inches.                  Diameter 0·65.

|  | Current direct. | Current reversed. |
|---|---|---|
| Position 1. | 630 | 262 |
| Position 2. | 645 | 235 |
| Position 3. | 660 | 202 |

In all these cases we have ample evidence of a polar action the reverse of that exhibited by diamagnetic liquids. These, I believe, are the first experiments on which the action of either liquid magnets or liquid diamagnets upon a suspended steel magnet has been exhibited.

Thus far then the following substances have been submitted to examination :—

| Diamagnetic bodies. | Magnetic bodies. |
|---|---|
| Bismuth. | Penrhyn slate. |
| Antimony. | Caermarthen slate. |
| Heavy glass. | Slate perpendicular to cleavage. |
| Calcareous spar. | Chloride of iron. |
| Statuary marble. | Sulphate of iron. |
| Phosphorus. | Carbonate of iron. |
| Sulphur. | Ferrocyanide of potassium. |
| Nitre. | Oxide of iron. |
| Wax. | Iron filings. |
| Liquids. | Liquids. |
| Distilled water. | Sulphate of iron. |
| Bisulphide of carbon. | Muriate of nickel. |
| | Muriate of cobalt. |

Every substance in each of these lists has been proved to be polar under magnetic influence, the polarity of the diamagnetic bodies being invariably opposed to that of the magnetic ones.

In his investigation on the supposed polarity of diamagnetic bodies, Mr. Faraday made use of a core of sixpenny pieces, and obtained with it the results he sought. Wishing to add the testimony of silver as a good conductor to that of copper, two cylinders were formed of sixpenny pieces, covered with paper, and submitted to experiment. The following are the results obtained :—

### XXVIII.

#### Silver cylinders (sixpenny pieces).

| Current direct. | Current direct. | Current direct. |
|---|---|---|
| Position 1. 724 | Position 2. 774 | Position 3. 804 |

The action here was prompt and energetic, strongly contrasted with the neutrality of copper ; but the deflection was permanent, and could not therefore be the result of induced currents. Further, it was a deflection which shows magnetic polarity, whereas pure silver is feebly diamagnetic. The cylinders were removed and examined between the poles of an electro-magnet ; they proved to be magnetic.

On observing this deportment of the silver, I tried the copper cylinders once more. The results with a direct current were,—

### XXIX.

| Position 1. 766 | Position 2. 767 | Position 3. 768 |
|---|---|---|

Here almost the same neutrality as before is evidenced.

Deeming that the magnetism of the cores of silver was due to

magnetic impurity attaching itself to the paper which covered them, a number of fourpenny pieces were procured, washed in ammonia and water, and enclosed in thin glass tubes. The following were the results :—

### XXX.

#### Silver cylinders (fourpenny pieces).

| Current direct. | Current direct. | Current direct. |
|---|---|---|
| Position 1. 490 | Position 2. 565 | Position 3. 660 |

Here also we have a very considerable action indicative of magnetic polarity. On examining the cylinders between the poles of an electro-magnet, they were found decidedly magnetic. This, therefore, appears to be the common character of our silver coins. The tubes which contained the pieces were sensibly neutral.

Knowing the difficulty of demonstrating the existence of diamagnetic polarity in ordinary insulators, M. Matteucci suggested that insulated fragments of bismuth ought to be employed, the insulation being effected by a coat of lac or resin. I constructed a pair of cylinders in accordance with the suggestion of M. Matteucci. The following are the results they yielded with a direct current :—

### XXXI.

| Position 1. 730 | Position 2. 750 | Position 3. 768 |
|---|---|---|

Here we have a very marked action, but the polarity indicated is magnetic polarity. On subsequent examination, the cylinders proved to be magnetic. This was due to impurities attaching themselves to the resin.

But the resin may be done away with and the powdered metal still rendered an insulator. This thought was suggested to me by an experiment of Mr. Faraday, which I will here describe. Referring to certain effects obtained in his investigations on supposed diamagnetic polarity, he writes thus :—" If the effect were produced by induced currents in the mass, division of the mass would stop these currents and so alter the effect; whereas, if produced by *a true diamagnetic polarity*, division of the mass would not affect the polarity seriously or in its essential nature. Some copper filings were therefore digested for a few days in dilute sulphuric acid to remove any adhering iron, then well-washed and dried, and afterwards warmed and stirred in the air, until it was seen by the orange colour that a very thin film of oxide had formed upon them ; they were finally introduced into a glass tube and employed as a core. It produced no effect whatever, but was as inactive as bismuth." (Exper. Resear. 2658.)

Now when bismuth is powdered and exposed to the action of

the air, it very soon becomes tarnished, even without heating. A quantity of such powder was prepared, and its conducting power for electricity tested. The clean ends of two copper wires proceeding from a battery of Grove were immersed in the powder; but though the wires were brought as near as possible to each other, short of contact, not the slightest action was observed upon a galvanometer placed in the circuit. When the wires touched, the needle of the galvanometer flew violently aside, thus proving that the current was there, but that the powder was unable to conduct it. Two glass tubes were filled with the powder and submitted to experiment. The following results were obtained:—

### XXXII.
#### Cylinders of Bismuth Powder.

Length 3 inches.                    Diameter 0·7.

|              | Current direct. | Current reversed. |
|--------------|-----------------|-------------------|
| Position 1.  | 640             | 230               |
| Position 2.  | 625             | 245               |
| Position 3.  | 596             | 260               |

These deflections are the same in kind as those obtained with the cylinders of massive bismuth. This experiment responds perfectly to the conditions proposed by Mr. Faraday. We have here no cessation of action. The division of the mass does not affect the result seriously or in its essential nature, and hence the deportment exhibits the characteristics of " a true diamagnetic polarity."

In summing up the results of his inquiry on this subject, Mr. Faraday writes thus:—" Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity, either in my own experiments, or in the repetition of those of Weber, Reich and others. . . . . It appears to me also, that, as magnetic polarity conferred by iron or nickel in small quantity, and in unfavourable states, is far more easily indicated by its effects upon an astatic needle, or by pointing between the poles of a strong horseshoe magnet, than by any such arrangement as mine or Weber's or Reich's, so *diamagnetic polarity would be much more easily distinguished in the same way.*" I was struck, on reading this passage, to find how accurately the surmise has been fulfilled by the instrument with which the foregoing experiments were made. In illustration of the powers of this instrument, as compared with that made use of by Mr. Faraday, I may be permitted to quote the following result from his paper on supposed diamagnetic polarity so often referred to:—" A thin glass tube, $5\frac{1}{2}$ inches by three-quarters of an inch, was filled with a saturated solution of proto-

sulphate of iron, and employed as an experimental core; the velocity given to the machine at this and all average times was such as to cause five or six approaches and withdrawals of the core in one second; yet the solution produced no sensible indication on the galvanometer." Referring to Table XXV., it will be seen that the instrument made use of in the present inquiry has given with a solution of protosulphate of iron a deflection amounting to no less than one hundred divisions of the scale. Mr. Faraday proceeds :—" A tube filled with small crystals of protosulphate of iron caused the needle to move about 2°. . . . . Red oxide of iron produced the least possible effect." In the experiments recorded in the foregoing pages, the crystallized sulphate of iron gave a deflection of nearly two hundred divisions of the scale, while the red oxide gave a deflection as wide as the helices would permit, which corresponds to about eight hundred divisions of the scale. The correctness of Mr. Faraday's statement regarding the inferiority of the means first devised to investigate this subject, is thus strikingly illustrated. It might be added, that red ferroprussiate of potash and other substances, which have given us powerful effects, produced no sensible impression in experiments made with the other instrument.

Thus have we seen the objections raised against diamagnetic polarity fall away one by one, and a body of evidence accumulated in its favour, which places it among the most firmly established truths of science. This I cannot help thinking is mainly to be attributed to the bold and sincere questioning of the principle when it seemed questionable. The cause of science is more truly served, even by the denial of what may be a truth, than by the indolent acceptance of it on insufficient grounds. Such denials drive us to a deeper communion with Nature, and, as in the present instance, compel us through severe and laborious inquiry to strive after certainty, instead of resting satisfied, as we are prone to do, with mere probable conjecture.

Royal Institution, November 1855.

XXIV. *An attempt to investigate the causes of the Intervals between High-water and Slack-water, and between the turn of Tide near the Shore and in the Offing. By* C. MARETT, *M.A., Trinity College, Cambridge*\*.

IT is well known to all accurate observers on the subject, that in tidal channels and in tidal rivers, the turn of tide, or slack-water, never exactly coincides with high-water, and that the tide near the shore always stops and turns before the tide in mid-

\* Communicated by the Author.

channel; but as far as I am aware, no attempt was made to give a satisfactory explanation of these phænomena until the publication, by the present Astronomer Royal, of the article on Tides and Waves in the *Encyclopædia Metropolitana.* In that article, very ingenious explanations of many of the phænomena in question are given from the analytical results of most unpromising hypotheses,— hypotheses admitted to be so much at variance with the real state of things, that the coincidence of the results is very extraordinary and unexpected. Though undoubtedly these results would take place on the hypotheses alluded to, I now venture to offer another explanation of the same, or of some of the same phænomena, founded upon considerations which I have been unable to reconcile with the above-mentioned hypotheses. They may, however, hereafter be reconciled, and it will be very singular if they cannot, and if it should turn out that two independent causes produce the same result; for it can hardly be doubted that the actions explained in this paper do really take place, and must produce some effect on the tidal motions.

### Explanation of Tide and Half-tide.

Let us suppose A to be an estuary or the open sea, in which the tide rises and falls alternately and uniformly 12 feet in six hours, this assumption being made for the sake of simplicity. Let R be a reservoir or inland sea in communication with A by the channel C, and of indefinite size, *i. e.* so large that the quantity of water flowing through C in six hours does not perceptibly affect



the general level in R; it is then obvious that in time the water in R will attain the mean level of the water in A, *i. e.* 6 feet above low-water mark in A. Let us now suppose it low water in A at $0^h$; the stream will then be ebbing out of R through C with the velocity due to the fall of 6 feet: as the tide in A rises the water in C will rise, but will continue to ebb from R through C till the water in A attains its mean height of 6 feet, which will be at $3^h$. The stream will then turn and begin to flow through C into R, rising at the same time till $6^h$, when it will be high water in A; the water in A will then begin to fall, but will continue to flow into R till $9^h$, when the water in A attains its mean level; the stream will then turn in C and ebb, the water falling at the same time till $12^h$,
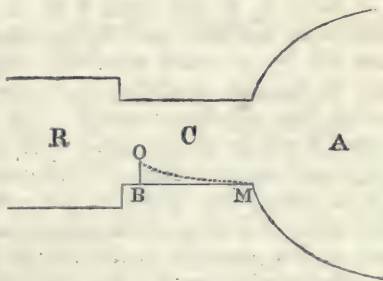
when the series will recommence.  We have therefore in C—

<div style="text-align:center">

Tide flowing from 3 till  9
... ebbing from 9 till  3
... rising from  0 till  6
... falling from  6 till 12

</div>

which is the system usually called tide and half-tide, and is explained (*Encyc. Metr.* ' Tides and Waves,' §§ 184, 507) upon independent considerations founded on the motions of water in waves.

### *Explanation of High-water occurring before the turn of tide.*

Let us now suppose that R is of limited area, which seems to be the ordinary case of a river or channel, and that the water flowing through C in six hours will affect the level of the water in R 6 feet; for the sake of simplicity let us suppose the rising and falling of the water in R to be uniform, viz. one foot per hour. Then at low water in A, the height of the water in R will be 4 ft. 6 in., as I shall proceed to show; for, assuming it to be so and the time to be $0^h$, the water in A will then begin to rise, and the water in R will fall, ebbing through C, till $1^h$ $30^m$, at which time the levels will be the same in both, having fallen 1 ft. 6 in. in R and risen 3 ft. in A.  The stream in C will then turn and flow into R, the water in R rising at the same time till $6^h$, at which time the height of the water in R will be 7 ft. 6 in., in A 12 ft.  The water in A and C will then begin to fall, but still flow through C till $7^h$ $30^m$, when the water in A and R will be at the same level, viz. 9 feet : the stream in C will then turn and ebb, and will continue to ebb and also to fall till 12 hours, when it will be low-water in A and 4 ft. 6 in. in R. The series will then recommence, and the original hypothesis, that at low water in A the height would be 4ft. 6 in. in R, is therefore correct, as it is the only permanent system.  We shall have therefore in C,—Tide flowing from $1^h$ $30^m$ to  $7^h$ $30^m$,

<div style="text-align:center">

... ebbing  ...  $7^h$ $30^m$ to  $1^h$ $30^m$,
... rising  ....  $0^h$ $0^m$ to  $6^h$ $0^m$,
... falling  ...  $6^h$ $0^m$ to $12^h$ $0^m$,

</div>

high-water and low-water each occurring one hour and a half before slack-water.  Similarly, if R were smaller, it might be shown that high-water would be nearer slack-water, and ultimately, when R becomes nothing, or at the end of the channel C, high-water would coincide with slack-water.—*Tides and Waves,* § 335.

Now if we take any section of a river, we may regard that section as C, and the portion of the river above it as R, and we have an explanation of the phænomenon observed in rivers,

modified of course by the quantity of fresh water in ordinary rivers.

That this is the true theory of the interval between high-water and slack-water, may also be inferred in a simpler manner from the following considerations. Let an observer station himself at any point in a tidal river, say at Putney Bridge, just before high-water : the tide will then be flowing up strongly in the middle of the river, slowly along the shore (see below) ; at high-water it will cease flowing along the shore and rise no higher, and in a short time will begin to fall, but will still run up in the middle. Now we know that high-water at Richmond and other places will not take place for more than an hour after high-water at Putney, and that the level of the water at Richmond is at this time lower than at Putney. But where is the water to come from which makes high-water at Richmond ? obviously it must come from below through Putney Bridge : and it is the water flowing up to make high-water at Richmond which causes the middle of the river to continue flowing at Putney after high-water : and when a sufficient quantity of water has flowed to make the level at Richmond as high or a little higher than at Putney, the tide will turn at Putney.

### *Explanation of the turn of tide near the shore before it turns in the offing.*

Let us now suppose an obstacle OB in the channel C, then whilst the tide is flowing, the water in OBM will not run with the same velocity as that in the main channel, but will merely have the velocity necessary to bring sufficient water into OBM, so that the water in OBM may continue to rise with the rise of tide. Now when the tide has ceased to rise, the water in OBM will cease to run at all, though, as before shown, the water in C will continue to flow : and when the tide begins to fall, the water in OBM will ebb (with the velocity necessary to take the water out of OBM, at such a rate as that the water in OBM may fall with the fall of tide), though the water in C will still continue to flow. It is obvious that OBM is a representation of the ordinary slack-water along the margin of rivers, occasioned either by actual projections or by the friction of the shore and shoals, or by the set of tide.

I add a few observations taken by me at Putney on a very calm afternoon, 9th of Nov. 1851, when there were many leaves, &c. floating on the stream. There was nothing particularly remarkable in the tide, but I am not aware that a similar set of observations are to be found in print, and the study of a tide of this sort may be instructive.

3$^h$ 0$^m$ P.M. Tide ceased along shore, flowing in the middle.
3$^h$ 15$^m$ ,, Ebbing close to the shore and falling.
3$^h$ 30$^m$ ,, Tide turned in the middle, having fallen about
15 inches. The margin of ebbing tide gradually grew wider on both sides of the river till it covered the whole river. The water at each point was not at rest more than 3$^m$.
3$^h$ 40$^m$ ,, Tide ebbing along shore; faster about 20 yards off; not so fast in the middle.
4$^h$ 5$^m$ ,, Tide in full ebb, fastest in the middle: backwaters along shore; having sunk about 3 feet 6 inches.

### XXV. *Chemical Notices from Foreign Journals.* *By* E. ATKINSON, *Ph.D.*

[Continued from p. 59.]

CANNIZARO found that hydruret of benzoyle, $C^{14} H^6 O^2$, was converted by the action of an alcoholic solution of potash into benzoic acid, $C^{14} H^6 O^4$, and the alcohol corresponding to it, $C^{14} H^8 O^2$. According to Cannizaro and Bertagnini*, the hydruret of anisyle undergoes, under the same circumstances, a like change, being converted into anisic acid, $C^{16} H^8 O^6$, and into an alcohol corresponding thereto, $C^{16} H^{10} O^4$, anisic alcohol.

Anisic alcohol distils without decomposition at 250°, and melts at 23°. It crystallizes in hard, white, brilliant needles. By oxidizing agents, and also when left in the air in contact with platinum black, it is converted first into hydruret of anisyle, and then into anisic acid. Anisic alcohol, when warm, dissolves several salts, as benzoate and acetate of potash ; and organic substances, as salicine, hippuric acid ; on cooling, these crystallize out. With potassium it forms a compound, hydrogen being liberated ; treated with hydrochloric acid gas, a body is obtained which appears to be the chlorine compound of the radical contained in the anisic alcohol.

The same chemists endeavoured to produce from hydruret of salicyle (salicylous acid), $C^{14} H^6 O^4$, the corresponding alcohol, but without success.

Casselmann† makes some interesting contributions to our knowledge of the oxychlorides. It was his object, in investigating these bodies, to endeavour to replace the oxygen contained in them by chlorine ; and as the chloride of phosphorus,

* *Il Nuovo Cimento,* vol. i. p. 99.
† Liebig's *Annalen,* May 1856, p. 213.

PCl⁵, corresponding to the oxychloride, $PO^2 Cl^3$, is known, the latter substance was chiefly used in the research.

He expected that by acting on oxychloride of phosphorus with substances which, under certain circumstances, readily exchange chlorine for oxygen, the pentachloride of phosphorus would have been obtained. In this direction the comportment of the chlorides of aluminium, magnesium, and zinc, and of the bichloride of tin, was investigated, but without the expected results. On the other hand, interesting compounds of oxychloride of phosphorus with these chlorides were obtained, which Casselmann describes at length.

The compound of oxychloride of phosphorus with bichloride of tin, $2Sn Cl^2 + PO^2 Cl^3$, he had previously described. It forms large colourless crystals, and when pure, distils unchanged. On account of the readiness with which this compound, and indeed all the compounds of this nature, attract moisture, they are extremely difficult to get quite pure; their preparation is effected in an apparatus by which the access of moisture is prevented.

Berzelius viewed oxychloride of phosphorus as a compound of phosphoric acid and pentachloride of phosphorus,

$$5PO^2 Cl^3 = 2PO^5 + 3PCl^5.$$
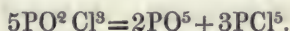
Such a compound would give with bichloride of tin, phosphate of tin, and the compound of pentachloride of phosphorus with bichloride of tin ($PCl^5 + 2Sn Cl^2$), previously described by Casselmann. But as the compound of oxychloride of phosphorus with bichloride of tin may be volatilized, no phosphate of tin can be contained in it, and Berzelius's view is hence not correct.

Oxychloride of phosphorus exhibits a strong tendency to combine with bichloride of tin. When brought into contact with the compound of bichloride of tin and bichloride of sulphur, $SnCl^2 + 2SCl^2$, the compound of oxychloride of phosphorus with bichloride of tin is obtained, bichloride of sulphur being set free.

When chloride of aluminium is mixed with oxychloride of phosphorus in a sealed tube, a white mass is obtained, which on heating, dissolves in the excess of oxychloride of phosphorus; and on cooling, the compound of oxychloride of phosphorus with chloride of aluminium is deposited in clear colourless needles. Its formula is $Al^2 Cl^3 + PO^2 Cl^3$.

With chloride of magnesium a compound is obtained which has the formula $2MgCl + PO^2 Cl^3$.

With chloride of tin a compound crystallizing in rhombic plates is obtained.

With metallic oxides the oxychloride of phosphorus decomposes, so that a metallic phosphate and a metallic chloride are formed. It cannot therefore be considered as phosphoric acid in which

oxygen is replaced by chlorine; for if so, it ought to combine directly with the metallic oxide.   Oxychloride of phosphorus in its compounds with metallic chlorides has the greatest similarity with pentachloride of phosphorus, and ought rather to be considered as this body, in which chlorine is replaced by oxygen.

Mixed with oxychloride of chromium, an action takes place which may be thus expressed :—

$$5\,PO^2\,Cl^3 + 12\,Cr\,O^2\,Cl = 18\,Cl + 5\,PO^5 + 3\,Cr^2\,O^3 + 3\,Cr^2\,Cl^3.$$
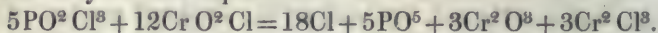
Casselmann also made experiments to ascertain the existence of the oxygen radical, assumed by Gerhardt to exist in oxychloride of phosphorus.   He was led to the conclusion, that, as is the case with the compounds of phosphorus with chlorine, phosphorus is the only electro-positive element.

Casselmann is making similar experiments with the chloride of benzoyle, $C^{14}\,H^5\,O^2$, Cl, and the chloride of acetyle, $C^4\,H^3\,O^2$, Cl.

Berzelius described a double salt of phosphate of lithia and phosphate of soda obtained by mixing a lithion salt with phosphate of soda.   This was used for the detection and estimation of lithia until Rammelsberg showed that the composition of the salt varied materially.   He viewed it as

$$\left.\begin{array}{c} 3\,NaO \\ 3\,LiO \end{array}\right\} PO^5,$$
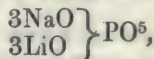
and considered the two bases as isomorphous and capable of replacing each other.

Mayer* made numerous attempts to prepare this salt, but invariably obtained a salt, which, after being well washed out, was tribasic phosphate of lithia.

Phosphate of lithia forms a heavy white crystalline powder, similar in appearance to phosphate of soda and magnesia.   It is soluble in 2539 parts of pure water, and in 3920 parts of ammoniacal water.   Mayer founded on the insolubility of this salt a process for the separation of lithia from the other alkalies, which, if it does not satisfy all the requirements of a good analytical method, seems better than the methods usually employed.

Kopp has an article† on the boiling-points of corresponding chlorine and bromine compounds, and on the formulæ of silicium and titanium compounds, which bear especial reference to a recent paper by Hofmann on the formulæ of titanium compounds.

In comparing the boiling-points of various corresponding bromine and chlorine compounds, Kopp was led eight years ago to the conclusion, that the former, when they contain $Br_x$ in the

* Liebig's *Annalen*, May, p. 193.                † Ibid. p. 265.

place of $Cl_x$, boil also by $x \times 32$ degrees higher than the latter. From this, he reasoned, a criterion was obtained as to how many equivalents of chlorine or bromine were contained in corresponding bromine and chlorine compounds. As bromide and chloride of silicon differ by $3 \times 32$ degrees in their boiling-points, Kopp inferred that their formulæ were respectively $Si\,Br^3$ and $Si\,Cl^3$.

The observations on boiling-points made at that time in corresponding chlorine and bromine compounds justified the assumption of that relation, but Kopp thinks now that that remark in its original generality is no longer true. In the last few years many new compounds have been discovered whose boiling-points do not agree with this rule, and the boiling-points of many bodies known at that time have since been determined in a more accurate manner. Kopp gives a list of such boiling-points, from which it appears, that, although in many cases of corresponding compounds containing bromine or chlorine the difference of boiling-points is a constant one, yet this is not universally the case, and it cannot be said that the difference of boiling-points is always the same for the same difference of formulæ. Hofmann has lately announced that the boiling-point of bromide of titanium differs from that of chloride of titanium by $95°$, $= 3 \times 31°\cdot7$, and reasons from this that the formula of bromide of titanium is $Ti\,Br^3$, and of the chloride, $Ti\,Cl^3$.

Kopp says, that although the difference in the boiling-points of chloride and bromide of silicon affords a support to the idea that their formulæ are $Si\,Cl^3$ and $Si\,Br^3$, yet it by no means proves it. From a purely chemical point of view we cannot decide which of the formulæ, $Si\,Cl^3$, $Si\,Cl^2$, $Si\,Cl$, for chloride of silicon is correct. Relations are found to exist between the chemical composition and certain physical properties, some of which may be urged for the formula $Si\,Cl^3$, and some for the formula $Si\,Cl^2$ or $Si^2\,Cl^4$.

In the case of titanium, the analogy of several of its compounds with tin, the atomic weight of which must be considered as fixed, and especially the isomorphism of rutile, $TiO^2$, and of native tinstone, $SnO^2$, as well as of Brookite, and of the crystallized oxide of tin obtained by M. Daubrée, appear to establish its equivalent, and the formula of many of its compounds. That the bromides and chlorides of titanium and silicon exhibit the differences in boiling-points is undoubtedly interesting; but it does not appear allowable to change the formulæ $Ti\,Cl^2$, $Ti\,Br^2$, &c., which are established on sure foundations, to $Ti\,Cl^3$, $Ti\,Br^3$, only on account of a difference in the boiling-points of two compounds which has no connexion with any general rule. Relations between the physical qualities and chemical composition may be consulted for the determination of atomic weights and of for-

mulæ, where chemistry does not herself possess the means of fixing these weights and these formulæ in a relatively certain manner. But formulæ which only express a relation concerning a single physical quality cannot be accepted as chemical formulæ, especially if they disagree with indubitable chemical analogies.

Reynoso proposed a method for the determination of phosphoric acid, which consisted in combining the phosphate with a known quantity of metastannic acid. By subtracting from the weight of the compound of phosphoric acid and metastannic acid formed, the metastannic acid used, the quantity of the phosphate is obtained. Although an excellent method, it has many difficulties which hinder its application. To remove these, and to make the method applicable to all cases, Reissig* proposes a modification of it, which consists in separating the phosphoric acid from the metastannic acid, and determining it directly as phosphate of magnesia and ammonia.

Schlossberger† has detected crystals of oxalate of lime in the Malpighian vessels of the spider.

Piria has lately shown that salicylic acid is bibasic. From this point of view Limpricht ‡ has submitted several compounds of this acid to an investigation. Salicylamide, regarded as
$$\left.\begin{array}{c} C^{14}\,H^5\,O^4 \\ H \\ H \end{array}\right\} N,$$
must be considered, on the bibasic view, as salicylamic acid,
$$\left.\begin{array}{c} NH^2\,(C^{14}\,H^4\,O^2) \\ H \end{array}\right\} O^2,$$
and Limpricht has prepared several of its salts. By the action of heat, salicylamic acid loses 2 equivs. of water, and is converted into salicylimide,

$$C^{14}\,H^7\,NO^4 - 2HO = C^{14}\,H^5\,NO^2.$$

The æthyle-salicylamic acid was also obtained by the action of ammonia on methylæthyle salicylic æther. Limpricht is still engaged in the investigation.

Kolbe § describes a new method for the formation of benzoic aldehyde. Since the researches of Chiozza and Gerhardt, the existence of compound radicals containing oxygen has been placed out of doubt, and the benzoyle theory of Liebig and Wöhler is again generally received. According to this, benzoic acid, chloride, hydruret, and cyanide of benzoyle are compounds of the radical benzoyle, $C^{14}\,H^5\,O^2$, or $(C^{12}\,H^5)\,C^2\,O^2$.

If, now, the hydrogen in hydruret of benzoyle, $C^{14}\,H^5\,O^2$, H,

* Liebig's *Annalen*, June, p. 339.          † Ibid. vol. xcviii. p. 354.
‡ Ibid. May, p. 256.          § Ibid. June, p. 344.

really occupies the same place as the chlorine in chloride of benzoyle, $C^{14} H^5 O^2$, Cl, this latter, or some other haloid compound of benzoyle, ought, by an appropriate action of hydrogen in the nascent state, to be converted into hydruret of benzoyle,

$$C^{14} H^5 O^2, Cl + 2H = C^{14} H^5 O^2, H + HCl.$$
Chloride of benzoyle.        Hydruret of benzoyle.

The ready decomposability of chloride of benzoyle renders it unfitted for this reaction, but the change takes place with cyanide of benzoyle; for when it is treated with hydrochloric acid, and granulated zinc added, decomposition of the cyanide takes place, the odour of hydrocyanic acid is evolved, and the mass contains a large quantity of hydruret of benzoyle, which is separable by distillation.

The same change occurs when cyanide of benzoyle is treated with mercury and hydrochloric acid.

Kraut* found that toluylic acid, when taken into the system, gives rise to the formation of a new acid. The toluylic acid was taken in doses of several grammes at a time without any perceptible effect on the organism; the urine had afterwards a distinctly acid reaction. The new acid, which Kraut names *toluric acid,* is obtained from the urine by the methods used for extracting hippuric acid from that source.

Toluric acid crystallizes in the rhombic system in hard lustrous crystals. It is easily soluble in alcohol and in boiling water, from which it crystallizes on cooling. It decomposes the earthy carbonates, forming with the bases crystalline salts. The formula of the acid is $C^{20} H^{11} NO^6$, and of its salts $C^{20} H^{10} NO^6$. Treated with hydrochloric acid, it undergoes a change entirely analogous to hippuric acid under the same circumstances, being converted into toluylic acid and alcohol.

Toluylic acid undergoes, hence, in passing through the organism, a similar change to benzoic acid; with loss of water it assimilates the elements of glycocoll: thus—
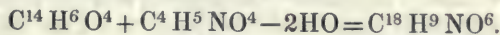
$$C^{14} H^6 O^4 + C^4 H^5 NO^4 - 2HO = C^{18} H^9 NO^6.$$
Benzoic acid.   Glycocoll.        Hippuric acid.

$$C^{16} H^8 O^4 + C^4 H^5 NO^4 - 2HO = C^{20} H^{11} NO^6.$$
Toluylic acid.   Glycocoll.        Toluric acid.

Kraut confirms the statement of Hofmann, that cuminic acid passes through the body unchanged.

Professor Schmidt† of Dorpat has investigated the boracic acid fumaroles of Monte Cerboli in Tuscany. In order to check

* Liebig's *Annalen,* June, p. 360.        † Ibid. p. 273.

the assertion of Payen, that the fumarole vapours, before their passage through the waters of the lagoons, contain no boracic acid, the following experiment was made. A large glass funnel was fastened over one of the jets of vapour issuing from the ground, and connected by means of a leaden pipe and several long wide glass tubes with empty glass balloons, which served as condensers. The gas evolved was a mixture of much carbonic acid with a small quantity of sulphuretted hydrogen. Small quantities only of nitrogen, and no oxygen were found. The liquid condensed in the receiver reddened litmus paper, but rendered turmeric paper brown; and a quantity of it evaporated to dryness and treated with sulphuric acid, imparted to the alcohol flame the characteristic green colour of boracic acid, proving that boracic acid exists preformed in the fumarole vapours. The condensed liquid contained, besides, carbonic acid and ammonia in considerable quantity. Distinctly estimable amounts of silica, baryta, iron, alumina, lime and magnesia, were also found. From a series of experiments, Schmidt supposes that the fumarole liquid contains about 0·1 per cent. of boracic acid, and thinks that it is in the form of borate of baryta.

Hessel* found that by the addition of a certain quantity of calcined gypsum to muddy wines, they were clarified and rendered somewhat stronger, retaining their original taste and flavour. Gypsum has the same effect on muddy beer, with the exception that it is rendered more bitter.

Two important methods for the artificial production of urea are given by Natanson†. He shows that carbamide and urea are identical. When carbonic æther is heated with excess of ammonia in a closed tube to 100° C., urethane alone is formed; but if the temperature of the tube be raised to 180°, the boiling-point of urethane, it is converted by the excess of ammonia into urea. In the empty parts of the tube a sublimate of undecomposed urethane is deposited: the aqueous solution contains urea. If this solution be evaporated to dryness and kept some time at 100° C., the urethane volatilizes and urea remains behind.

In 1838 Regnault obtained, by the action of phosgene gas on ammonia, a white saline mass, which comported itself as a mixture of carbamide and sal-ammoniac. In this mass urea must probably have been contained; but if the gases had not been well dried, its quantity would have been small in comparison to the other substances, and it might easily have escaped detection.

Natanson prepared the phosgene gas by passing carbonic oxide

* Liebig's *Annalen,* June, p. 334.        † Ibid. p. 287.

through perchloride of antimony. It was conducted along with ammonia, both gases being quite dry, into a capacious vessel. The mass obtained was treated with absolute alcohol, the alcoholic solution evaporated to dryness, and the residue dissolved in a small quantity of cold water. On the addition of nitric acid, the characteristic nitrate of urea is obtained. The presence of urea was confirmed by other reagents.

Natanson* found that by the action of chloride of elayle on ammonia, a base, the oxide of acetylammonium, was produced. The investigation of this he has resumed, and communicates now his results. He finds that oxide of acetylammonium splits up at a higher temperature into acetylamine and water,—

$$\left.\begin{array}{c} C^4\,H^3 \\ H \\ H \\ H \end{array}\right\}NO,\ HO = \left.\begin{array}{c} C^4\,H^3 \\ H \\ H \end{array}\right\}N + 2HO.$$
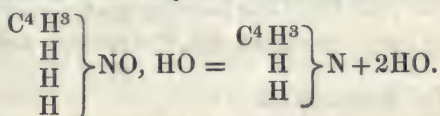
An alcoholic solution of pure hydrated oxide of acetylammonium is heated in a retort until all the alcohol has distilled off; if the temperature be then raised, drops of water containing acetylamine dissolved pass over. This water results from the ·decomposition; and after it has almost ceased to come over, the temperature is raised to 220° C., at which point yellowish oily drops come over, which consist almost entirely of pure acetylamine. Above that point other products of decomposition are formed, and the acetylamine passes over in very small quantity. On rectification, the acetylamine was found to boil at 218° C. Pure acetylamine is an almost colourless, oily liquid, of peculiar adherent odour. Its specific gravity is 0·975. At ordinary temperatures it smells like aldehyde-ammonia; the vapours of boiling acetylamine smell like pure aniline. Acetylamine is miscible with alcohol and water, but not with æther. On dry litmus *it has no action,* but on the addition of water the paper becomes blue. It has a caustic taste. Sodium has no action on it. Acids form with it salts which have all the characters of the salts of acetylammonium. On the addition of potash to a solution of the sulphate, the characteristic odour of acetylamine is not perceived. Acetylamine is eliminated therefore in the form of the inodorous oxide of acetylammonium. In the air, acetylamine attracts moisture and carbonic acid; in contact with hydrochloric acid gas, white vapours are formed. With solutions of the metals, many characteristic reactions are observed.

When acetylamine is heated with iodide of æthyle, an iodide of the æthylated base is obtained. By treatment with potash

* Liebig's *Annalen,* June, p. 291.

this yields a brown oil, which is a base, and forms compounds with acids. It is probably æthylacetylamine, $\left.\begin{array}{c} C^4 H^5 \\ C^4 H^3 \\ H \end{array}\right\} N.$

By the action of chloride of elayle on aniline in closed tubes at 200° C., and subsequent solution of the contents of the tubes in water, a mixture is obtained of the hydrochlorates of aniline and of acetylaniline, which are separable by ammonia, aniline being precipitated. Acetylaniline, when purified, is a light brown, tasteless, and inodorous powder, which is soluble in alcohol and æther, but insoluble in water. It does not crystallize, and it forms with acids salts which are also uncrystallizable. It gives with bichloride of platinum a double salt. Acetylaniline is constituted according to the formula $\left.\begin{array}{c} C^4 \ H^3 \\ C^{12} H^5 \\ H \end{array}\right\} N,$ and its formation may be thus expressed:—

$$C^4 H^4 Cl^2 + 2C^{12} H^7 N = C^{12} H^7 N \ HCl + C^{16} H^9 N, \ HCl.$$

   Chloride of     Aniline.     Hydrochlorate     Hydrochlorate of
     elayle.                of aniline.      acetylaniline.
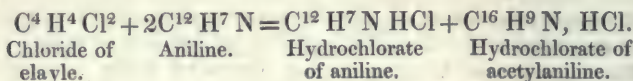
Compounds corresponding to the amides are also formed by acetylamine. If butyric æther be mixed with an alcoholic solution of acetylamine, the whole solidifies to a mass of fine crystalline needles. These are the acetylamide of butyric acid:—

$$\left(C^8 H^7 O^2 + \begin{array}{c} C^4 \ H^3 \\ H \end{array}\right\} N).$$
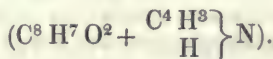
The comparison of the properties of acetylamine and of oxide of acetylammonium is an instructive contribution to the ammonium theory. Here the separation into the corresponding ammonia and water takes place at 150° C., while ordinary oxide of ammonium probably suffers this change below 0°. Acetylamine has all the characteristic properties of the ammonias, and, like them, has no action on perfectly dry litmus paper. On the addition of water and acids, it forms salts corresponding to the ammonia salts, losing its characteristic properties; but by the addition of strong bases is set free as the strongly alkaline oxide of acetylammonium. Natanson considers these relations as supporting an opinion already expressed by him, that the ammonias are the *neutral* products of decomposition of the oxides of ammonium, the real bases.

The same chemist describes a modification of Gay-Lussac's method of determining the specific gravity of vapours, in which very small quantities only of substance are required.

XXVI. *Remarks on Slaty Cleavage, and the Contortions of Rocks.* *By* Sir John F. W. Herschel, *Bart., F.R.S. &c.*

Dear Sir,                                     Collingwood, July 27, 1856.

LET me thank you for several instances of obliging attention in forwarding me copies of your scientific papers on diamagnetism, and quite recently of your lecture on the Slaty Cleavage, which appears to me a singularly happy explanation of that odd phænomenon.   I may mention an illustration of the same nature with some others you have introduced, which occurs in the manufacture of the coin of the realm.   One part of the process consists in "ringing" each piece of gold or silver coin (after receiving the impression).   Every piece which will not ring on dashing it against a stone is rejected and re-melted; and such are numerous.   Now the cause of this can be no other than minute air-blebs in the bars out of which the laminæ of metal are rolled by a flatting-mill, each bleb being flattened out into an infinitesimally thin film of air parallel to the faces of the coin, and so producing a solution of continuity, and of course destroying the harmonic vibration.

There are one or two points suggested by your views of the slaty cleavage which harmonize so entirely with certain ideas I have long held about geological stratification in general, that I cannot forbear mentioning them.   I refer to the contortions of strata which are usually accounted for (as Poulett Scrope has done) by the protrusive violence of igneous rocks crushing together and doubling up the strata.   To me it has always seemed that this feature might be more tranquilly produced thus :—
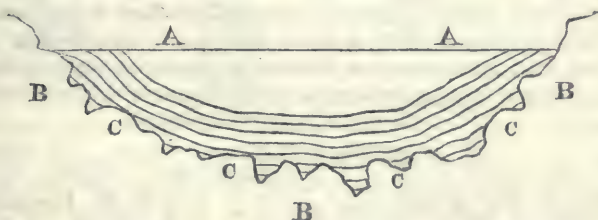


Suppose A A the ocean surface, and B B B its bed, forming a basin having irregularities C C in it.   Let sediment be deposited over the whole, uniformly, till the inequalities are filled in and the bottom is reduced to a, generally speaking, basin-like depression.   Then up to a certain point the friction of the bed will retain the deposited mud on its slope, and in this way strata nearly of uniform thickness (and parallel) will be formed.

But as the thickness of the whole increases, the weight will overcome the friction, and the still soft strata will yield inwards from all sides towards the central portion of the bed, increasing the thickness of the strata there, and at the same time crumpling up the strata into contortions, &c.   The whole affair may go on with any degree of slowness*.

Now as this process goes on, the middle thickness will increase, and that by a general lateral compression and vertical dilatation of the whole central mass.   And here we have the condition which your theory requires for the slaty cleavage, which (be it observed) *does not absolutely require violent force* (for mere force cannot produce any rearrangement of molecules), but only *inter-molecular movement* in a given fixed direction.   Thus we see the blebs in slow-moving lava currents drawn out in length ; and thus we see liquids, in which excessively fine spicular crystals are floating, assume a silky appearance when stirred or agitated (I have a most beautiful case of the kind under my eye at this moment) by the tendency of the particles to arrange themselves when in motion all in one direction, according to the laws, *not of pressure* BUT *of friction,* a distinction which is quite necessary to be borne in mind.   Infinitesimal films of mica disseminated through amorphous mud would afford a precise parallel to this case.

Your account of the fibrous structure in tough iron is quite beautiful, and perfectly accounts for the exceeding toughness and tenacity of bar iron formed by welding together smaller bars, &c.   In fact, such a bar is a rope of iron wire held together as the fibres of a hempen rope are, by lateral friction, and each portion being *per se* of trivial length compared with the whole.

I am sure you will excuse me for writing to you in this desultory manner, which perhaps I am not entitled to do ; but the fact is, that since reading, a great many years ago, Wells's little tract on dew, I have not met with anything which seems to bring so many loose-lying phænomena under a general and very simple principle.

I remain, dear Sir,

Yours truly,

*J. Tyndall, Esq., F.R.S. &c.*                    J. F. W. HERSCHEL.

---

* In forcing down the strata, while still in some degree yielding, down *the general slope* of the ocean bed, they must often be driven over its larger inequalities in a way very likely to contort and corrugate them ; and this, too, equally whether the softness be that remaining in them previous to their consolidation, or be produced anew by invasion of heat from below producing semifusion and weakening their support.

XXVII. *Preliminary Notice on the Electric Conducting Power of the Alkaline Metals. By* A. MATTHIESSEN, *Ph.D.*\*

OF the physical properties of the metals of the alkalies and alkaline earths very little is at present known; and having some of them at my disposal, I thought it would be interesting to study some of their most important properties. I have commenced, under the direction of Professor Kirchhoff, in the Heidelberg Physical Laboratory, with the electric conducting power of the alkaline metals, and have found that of potassium at ordinary temperature, compared with that of silver at $0° = 100$, to be—

|  |  |
|---|---|
|  | 20·1 |
| Of sodium . . . | 36·6 |
| Of lithium . . . | 18·7 |

The manner in which the wires were pressed and the method of observation I shall describe as soon as I have determined their conducting power at different temperatures, as well as that of the metals of the alkaline earths.

An interesting fact is the rapid diminishing of the conducting power at the point of fusion. With potassium it diminishes gradually between the temperatures 50° to 57° C.; whereas with sodium, suddenly at about 96°, as the following data show:—

| The conducting power of potassium is at— | | That of sodium is at— | |
|---|---|---|---|
| 40° C. . . | 17·21 | 75° C. . . | 24·44 |
| 49° C. . . | 16·44 | 85° C. . . | 23·41 |
| 53° C. . . | 14·81 | 95° C. . . | 22·42 |
| 58° C. . . | 12·91 | 97° C. . . | 16·23 |
| 56° C. . . | 12·21 | 110° C. . . | 15·36 |
| 57° C. . . | 11·54 | 120° C. . . | 14·17 |
| 70° C. . . | 11 |  |  |
| 80° C. . . | 10·65 |  |  |

Compared with silver at $0° = 100$.

This difference in the behaviour of these metals may be explained by the observation lately made by Regnault, in his paper "On the Specific Heat of Simple Bodies" (*Ann. de Chim. et de Phys.* vol. xlvi. p. 257), wherein he states that potassium does not pass directly from the liquid to the solid state, like sodium, but becomes first semi-fluid and then gradually solid, which fact is plainly shown by comparing the conducting power of the two metals at their fusing-points.

---

\* Communicated by the Author.

## XXVIII. *On Rubian and its Products of Decomposition*\*.
### *By* Edward Schunck, *F.R.S.*

### Part III.

### *Combined Action of Alkalies and Oxygen on Rubian.*

IN the preceding part of this paper† I have given an account
of the action of alkalies on rubian and of the products of
decomposition thereby formed. I have shown that these pro-
ducts are, with one exception, identical with those which the
action of strong acids gives rise to, the rubianine produced by
acids being merely replaced by rubiadine when alkalies are em-
ployed. Now though I found this to be in all cases the final
result of the action of alkalies, there still remained a possibility
of the existence of bodies intermediate between rubian and the
final products of decomposition. Such bodies do in reality exist,
but their formation is dependent, at least in part, on the simul-
taneous action of oxygen.

If to a solution of pure rubian baryta water be added in excess,
if a current of carbonic acid gas be passed through the solution,
until the whole or a great part of the baryta has been converted
into bicarbonate of baryta, and the filtered solution be left ex-
posed in a shallow vessel to the atmosphere, its surface becomes
covered in a short time with a thin film of a beautiful scarlet
colour, which, when viewed under a lens, is seen to consist of
small interlaced crystals. If this film be broken so as to cause
it to sink to the bottom, a new one is formed on the surface of
the liquid. This process continues for some time, but at last
ceases. On gently evaporating the solution, an additional quan-
tity of this red substance separates in thick flocks. After being
collected on a filter and washed with cold water, it appears of a
fine scarlet colour. On drying it becomes brown, but if then
reduced to powder the colour changes to a cinnabar-red. If it
be suspended while still moist in water, and a current of car-
bonic acid gas be passed through the liquid, it dissolves again
entirely, forming a yellow solution ; but if this solution be eva-
porated, its colour changes to red, and it again deposits red
flocks, after which it becomes almost colourless. The red flocks
contain two organic substances in combination with baryta. On
adding to them dilute sulphuric acid the former dissolve, giving
a yellow solution. The excess of sulphuric acid having been
removed by means of carbonate of lead, the solution is filtered,
and the insoluble matter left on the filter is treated several times

with boiling water, after which it generally retains only a slight pink tinge. The filtered solution deposits on evaporation a quantity of yellow crystalline needles. After evaporating to dryness there is left a yellow mass, the surface and edges of which have a dark yellowish-brown tinge. On adding to this mass a small quantity of cold water, part of it dissolves with a deep yellow or reddish-yellow colour, while a yellow powder remains undissolved. The latter consists of a peculiar acid, to which I shall give the name of *Rubianic Acid.* After being collected on a filter and washed with cold water, it is purified by solution in boiling water, to which a little animal charcoal may be added. On filtering the solution boiling hot and allowing to cool, it crystallizes in beautiful lemon-yellow needles, which, if the solution was at all concentrated, entirely fill the liquid. The substance dissolved by the cold water is left on evaporation in the shape of a reddish-yellow or brownish-yellow substance, resembling rubian itself in appearance and all its properties. As analysis showed it to be formed from rubian by the elimination of several equivalents of water, I shall call it *Rubidehydran.*

The liquid filtered from the red flocks, consisting of the baryta compounds of the two substances just named, has still a dark brownish-yellow colour, and contains a third organic substance in solution. In order to ascertain whether the whole quantity of the two former substances has been separated, and whether the solution still contains any unchanged rubian, it is well to add caustic baryta to the liquid, to supersaturate the latter with carbonic acid and to evaporate again. If no more red flocks separate on evaporation, but only a deposit of carbonate of baryta be formed, then the process is completed. On now adding to the filtered solution basic acetate of lead a red precipitate falls, while the liquid becomes colourless. The former, being separated by filtration and washed with water, is to be decomposed with sulphuric acid in the cold, and the excess of acid having been removed with carbonate of lead, sulphuretted hydrogen is passed through the filtered liquid, and the latter, after being filtered again from the sulphuret of lead, is evaporated to dryness, when it leaves a dark brownish-yellow substance, resembling rubian in appearance, but differing in being somewhat deliquescent. To this substance I will give the name of *Rubihydran.* The liquid filtered from the lead compound of this substance sometimes contains sugar, but this is entirely a secondary product of decomposition, formed at the cost of one or more of the primary products, and indicates the formation at the same time either of rubiadine or alizarine.

The same products of decomposition may be obtained by adding to the solution of rubian a solution of bicarbonate of

baryta made from chloride of barium and bicarbonate of soda,
and evaporating in contact with the air until red flocks begin to
appear, or by adding caustic baryta to the solution, allowing the
mixture to stand exposed to the air for some time and filtering,
when the insoluble baryta compound left on the filter will yield
rubianic acid and rubidehydran, and the liquid rubihydran.
The same process of decomposition takes place, if caustic soda,
ammonia, or lime water be added to a watery solution of rubian,
and the mixtures be allowed to stand exposed to the air for
some time.  On now adding chloride of barium to any one of
them a dark red precipitate falls, which being collected on a
filter, washed with water and then treated in the same manner
as the red flocks obtained by means of bicarbonate of baryta,
affords in each case rubianic acid and rubidehydran, while the
liquid contains rubihydran.  If a small quantity of any acid be
added to a watery solution of rubian, and if the acid be then
neutralized with carbonate of baryta, the small quantity of
bicarbonate of baryta formed is sufficient to induce a decom-
position of the rubian, for the filtered solution on exposure to
the air very soon begins to deposit red flocks, which consist of
rubianic acid and rubidehydran in combination with baryta.  It
is for this reason that I have recommended the employment of
carbonate of lead instead of carbonate of baryta for the purpose
of neutralizing the sulphuric acid used in purifying rubian\*.
Even oxide of lead is a sufficiently strong base to cause rubian
to undergo this process of decomposition, when oxygen is pre-
sent at the same time.  If rubian be precipitated from its
watery solution by means of basic acetate of lead, and the lead
compound be left exposed to the air for a short time, it will be
found no longer to contain unchanged rubian.  If the com-
pound be decomposed with sulphuric acid in the cold, and the
excess of acid be neutralized with carbonate of lead, the filtered
solution deposits during evaporation crystals of rubianic acid,
and leaves at last a brown deliquescent mass, which unless it be
strongly dried is with difficulty removed from the vessel con-
taining it†.  In short, whenever rubian is brought into contact
at the same time with oxygen and an alkaline or other strong
base, it undergoes decomposition.  Hence it follows, that in
preparing the so-called xanthine according to the methods pro-

* See Phil. Mag. for March 1852, p. 222.
† In purifying rubian by precipitation with basic acetate of lead, as de-
scribed in the first part of this paper, care must be taken to wash the pre-
cipitate with alcohol and not with water.  If the former be employed, de-
composition is almost entirely prevented, whereas in using water the rubian
in the precipitate undergoes complete decomposition during the short time
necessary for edulcoration.

posed by Kuhlmann, Berzelius, Runge and Higgin, as well as the ruberythric acid of Rochleder, the use either of alkaline earths or basic acetate of lead being prescribed by all these chemists, products of the decomposition of rubian must in every case be formed.

For the purpose of preparing the three bodies which result from this process of decomposition, it is not necessary to employ pure rubian. If madder be extracted with boiling water, and sugar of lead be added to the extract, a purple precipitate is produced, and ammonia being added to the filtered liquid, the whole of the rubian is precipitated together with some chlorogenine in combination with oxide of lead. The precipitate is decomposed with sulphuric acid in the cold, and the excess of the acid is removed by means of carbonate of lead. To the filtered solution a quantity of baryta water is to be added, and the baryta is then converted into bicarbonate of baryta by a stream of carbonic acid gas. The solution is left exposed to the air for some time and then slowly evaporated. The red flocks which are deposited are collected on a filter as they form, and after being treated in the same way as the flocks from pure rubian, yield rubianic acid and rubidehydran. The liquid filtered from these flocks contains chlorogenine as well as rubihydran, and the separation of the latter becomes therefore rather more difficult. This may however be effected by precipitating it with basic acetate of lead, filtering, washing the precipitate with water, then redissolving it in warm acetic acid, and again precipitating by means of a little ammonia. The last precipitate contains hardly any chlorogenine, and after being put on a filter and washed, is decomposed with sulphuric acid in the cold. The excess of acid is removed by carbonate of lead, the liquid is filtered, sulphuretted hydrogen is passed through it, it is again filtered from the sulphuret of lead, and on being evaporated leaves the rubihydran free from chlorogenine. Should it still contain a little of the latter substance, in which case its watery solution turns green on being boiled with muriatic or sulphuric acid, the precipitation with basic acetate of lead must be repeated. It frequently happens, that besides the usual products of decomposition, a quantity of rubiadine is also formed. In this case the red flocks deposited on evaporating the barytic solution yield, on being decomposed with sulphuric acid, besides the substances soluble in water, a yellow powder, which is insoluble both in cold and boiling water, and which consists of impure rubiadine; but this, like the sugar, which is also sometimes formed during the process, is without doubt a secondary product of decomposition. Indeed the formation of the one is most probably dependent on that of the other.

The process of decomposition just described is always accompanied by an absorption of oxygen. In order to ascertain the quantity of the latter which an alkaline solution of rubian was capable of absorbing, I took 4·2280 grms. of rubian, which after deducting the inorganic matter with which it was contaminated, was equivalent to 4·1049 grms. of the pure substance, dissolved it in hot water, poured the solution into a graduated tube, added to it a solution of about 6 grms. hydrate of baryta, filled the rest of the tube with mercury, and then inverted it over mercury. Oxygen gas was then introduced, and the liquid was from time to time agitated with the gas, in order to bring every portion, and especially the red flocculent precipitate produced by the baryta, and which sank to the bottom of the solution, into frequent contact with it. After 143 days I found that 147 cubic centimetres of gas had been absorbed. The contents of the tube were then removed and filtered. There remained on the filter a red baryta compound, which, on being treated as usual, yielded rubianic acid and rubidehydran, as well as a small quantity of alizarine. To the red liquid sulphuric acid was added; the excess of acid was removed with carbonate of lead, and sulphuretted hydrogen having been passed through the filtered liquid, the latter, after being filtered again from the sulphuret of lead, was submitted to distillation. The distillate was acid, and after being neutralized with carbonate of soda and evaporated, left a saline residue having all the characters of acetate of soda. Acetic acid is therefore another product formed in this process, but whether this acid is an essential product of decomposition or not, still remains doubtful. The residue of the distillation contained rubihydran, which was separated by precipitation with basic acetate of lead, as before described, and a little sugar, which was obtained from the liquid filtered from the lead precipitate. The sugar and the alizarine were probably secondary products of decomposition formed from the rubianic acid by the action of the alkali.

*Rubianic Acid.*—This substance really merits the name of an acid, for though its acid properties are not well marked, the fact of its giving crystallized compounds with the alkalies is a sufficient indication of the class in which it should be placed. It crystallizes from its watery solution in silky needles of a pure lemon-yellow colour, which when dry form a light, bulky interwoven mass. Sometimes it is slowly deposited from its watery solution in grains and masses of an indistinctly crystalline form, which have an orange tinge. This difference in appearance is due to some impurity, which may be removed by redissolving the acid in boiling water and adding a little animal charcoal, when the acid crystallizes rapidly from the filtered solution in

needles, as just mentioned. The watery solution has a light yellow colour, reddens litmus paper slightly, and has a distinctly bitter taste, though not so intensely bitter as that of a solution of rubian. It is soluble in alcohol, but not in æther. When heated on platinum foil it melts and then burns with a smoky flame, leaving a slight carbonaceous residue. When heated in a tube it melts to a brownish-red liquid, which on being allowed to cool becomes solid and crystalline, but on being heated again more strongly gives fumes, which condense on the colder parts of the tube to a liquid, which soon solidifies, forming a mass of shining needles. When slowly heated between two watch-glasses, there is formed on the upper glass a sublimate of shining orange-coloured crystals, having the appearance and all the properties of alizarine, while a considerable carbonaceous residue is left on the lower glass. Concentrated sulphuric acid dissolves rubianic acid easily even in the cold, forming a dark red solution, which on being boiled becomes of a dark reddish-brown colour, without evolving much sulphurous acid. If sulphuric acid be added to a watery solution of rubianic acid, and the solution be boiled, dark yellow or orange-coloured flocks begin to be deposited, which increase as the boiling continues. On allowing to cool they often separate in such quantities as to render the liquid quite thick. These flocks are quite uncrystalline, though sometimes small yellow crystalline grains are found among them, which consist of undecomposed acid, and disappear on continuing the action for some time longer. These flocks consist of alizarine almost in a state of purity. On dissolving them in alcohol and evaporating spontaneously, a quantity of beautifully crystallized alizarine is obtained. The liquid filtered from the flocks contains sugar, which is obtained with its usual appearance and properties on neutralizing the acid with carbonate of lead, filtering, passing sulphuretted hydrogen through the liquid, filtering again, and evaporating. Muriatic acid acts in precisely the same manner on rubianic acid as sulphuric acid. Nitric acid dissolves rubianic acid even in the cold, forming a yellow solution, which on being boiled disengages nitrous fumes and becomes colourless. The solution on evaporation leaves a brown syrup, which contains oxalic acid. Rubianic acid is not decomposed on being treated with boiling solutions of phosphoric, acetic, oxalic or tartaric acids; it merely dissolves in them, and crystallizes out again unchanged on the solutions cooling. If chlorine gas be passed through a watery solution of rubianic acid, the yellow colour of the solution slowly disappears. If there are any crystals of undissolved acid floating in the liquid, these disappear very slowly, and the gas must be passed through for a long time in order to effect their decomposition. The

solution now appears colourless, but slightly milky. No yellow
flocks are formed, as in the case of rubian, but the solution, on
standing for some time, usually deposits a quantity of brown-
ish-yellow crystals. If a solution of chloride of lime be added
to a watery solution of rubianic acid, the latter turns blood-red,
but in a few moments the colour disappears, and the solution
becomes quite colourless.

Rubianic acid dissolves in caustic potash and soda in the cold
with a lively cherry-red colour. If caustic potash be added to
a strong watery solution of the acid, nothing separates on stand-
ing; but if carbonate of potash be employed instead of caustic
potash, there is deposited almost immediately a quantity of
dark-coloured crystalline needles. These needles are the potash
salt. When collected on a filter, slightly washed with water and
dried, they form a beautiful puce-coloured silky mass. When
dried either in the water-bath or *in vacuo,* the colour changes to
a bright red, but after a few minutes' exposure to the atmo-
sphere the original colour is restored. Rubianic acid is so weak
an acid, that water alone is sufficient to separate it from its
combination with potash. When the potash salt is treated with
boiling water it dissolves with a red colour, but the solution on
cooling and standing deposits yellow crystals of the acid itself.
Mere treatment with cold water produces to some extent the
same effect; for if the salt be placed on a filter and washed with
cold water, numerous small yellow specks, caused by the sepa-
ration of the acid, become visible. When caustic or carbonate
of soda are added to a boiling watery solution of the acid, the
solution deposits on cooling a quantity of small bright red sphe-
rical grains, which generally form a mass so bulky as to render
the liquid thick and gelatinous. When this mass, which con-
sists of the soda salt, is placed on a filter, an almost colourless
liquid runs through, leaving the salt on the filter in a soft spongy
state, from its retaining mechanically a large quantity of water.
The round grains of which it consists, though they have a some-
what crystalline appearance, are not in reality crystalline. In
consequence of its sparing solubility in water, which is less than
that of the potash salt, it may be washed with cold water with-
out dissolving. When dry it is dark red, and gives a red powder.
It dissolves again in boiling water with a red colour, and the
solution on cooling forms a thick jelly, which however is con-
verted, on standing, into a mass of yellow crystalline grains con-
sisting of acid. If a solution of rubianic acid in caustic potash
or soda be supersaturated with any stronger acid, the solution
becomes yellow without depositing anything, but after some
time the acid separates in yellow crystals. But if the alkaline
solution, containing a considerable excess of alkali, be boiled, its

colour changes gradually from red to purple, and after prolonged boiling to violet, similar to that of a solution of alizarine in caustic alkali, the colour being generally so intense as to deprive the solution of its transparency. Acids now produce an immediate precipitate of dark yellow flocks, while the liquid becomes almost colourless. If the action of caustic alkali has not been continued long enough to produce a complete decomposition of the acid, then after some time crystals of the latter appear in the midst of the flocculent precipitate. The flocks consist of alizarine, but less pure than when the decomposition is effected by means of sulphuric or muriatic acid. On decomposing a considerable quantity of the acid by means of caustic soda, and treating the precipitated flocks in the same manner as those formed by the decomposition of rubian with acids or alkalies, I obtained, besides alizarine, a trace of verantine and a small quantity of a substance resembling rubianine or rubiadine*. The liquid filtered from the flocks contains sugar. Rubianic acid dissolves in caustic ammonia with the same colour as in caustic potash or soda, but with much greater difficulty than in the latter. The solution does not change its colour, however long it may be boiled, and on adding an excess of a stronger acid, the rubianic acid crystallizes out unchanged. On evaporating the ammoniacal solution to dryness it loses part of its ammonia, leaving some yellow crystals of acid surrounded by a red uncrystalline coating like gum. The latter dissolves in cold water, but on evaporation the solution again gives yellow crystals. If to a boiling watery solution of the acid carbonate of ammonia be added, the solution becomes red, and deposits on cooling crystals of the ammonia salt, which have the same colour and appearance as the potash salt. These crystals may be dried without losing their ammonia, but, like the other alkaline salts, are decomposed on redissolving them in boiling water, the solution depositing on cooling crystals of the acid.

Baryta water gives in a watery solution of rubianic acid a beautiful crimson flocculent precipitate, while the liquid becomes colourless. If carbonic acid be passed through the liquid the precipitate gradually dissolves, forming a clear yellow solution; but if this solution be left exposed to the air, its surface becomes covered with a red film, and on evaporation it deposits a quantity of red flocks, until at length the whole of the acid

---

* This substance is light yellow and crystalline. When heated it is volatilized, giving a sublimate of yellow shining needles and scales. It is insoluble in boiling water, but dissolves in boiling nitric acid, and crystallizes out of the solution again on cooling in yellow needles. It is precipitated from its alcoholic solution by acetate of copper, but not by acetate of lead.

seems to be again precipitated in combination with baryta. Lime-water produces in a watery solution of the acid a light red precipitate, while the liquid becomes colourless. If carbonic acid be now passed through the liquid the precipitate dissolves, forming a yellow solution, which on exposure to the air becomes red, but gives no red film, and on evaporation deposits no flocks, but leaves at last a red mass, which dissolves again in boiling water. Acetate of alumina and peracetate of iron produce no change in a watery solution of the acid. Nevertheless the latter is entirely removed from its solution by hydrate of alumina, to which it communicates a light red colour, and also by hydrated peroxide of iron, though not so easily as by alumina. Rubianic acid dissolves easily in a boiling solution of perchloride of iron, forming a dark greenish-brown liquid, which contains proto-chloride of iron. The solution deposits nothing on standing, and on evaporation only a small quantity of black powder separates, which is probably a compound of alizarine and oxide of iron. Acetate of copper gives in watery and alcoholic solutions of the acid a brownish-red precipitate, which is soluble in boiling acetic acid. Neutral acetate of lead turns the watery solution of the acid red, but produces no precipitate; basic acetate of lead, however, gives a copious red flocculent precipitate, just as in a solution of rubian. An alcoholic solution of the acid gives with an alcoholic solution of acetate of lead a light red precipitate, which after filtration of the alcoholic liquid dissolves in pure water. On adding nitrate of silver to an aqueous solution of the acid and boiling no change takes place, but on the addition of a few drops of ammonia a dark reddish-brown precipitate slowly subsides as a fine powder, which is soluble in an excess of ammonia, forming a red solution, which does not change, and shows no signs of any reduction taking place when it is boiled. On adding chloride of gold to a watery solution of the acid no change takes place even on boiling, but on the addition of a little caustic alkali metallic gold is deposited, partly as a brown powder, partly in shining scales. If rubianic acid be mixed with water to which a quantity of erythrozym has been added, the yellow crystals of the acid gradually disappear, and are converted into alizarine. The liquid is found to contain sugar.

It is impossible to dye with rubianic acid. Mordants acquire in boiling solutions of the acid no more colour than in solutions of rubian or rubianine.

On submitting the acid to analysis the following results were obtained :—

I. 0·4605 grm., formed by the action of bicarbonate of baryta on rubian, dried in the water-bath and burnt with chromate of lead, gave 0·9445 carbonic acid and 0·2250 water.

II. 0·4030 grm. of another preparation, obtained by the action of bicarbonate of baryta, gave 0·8240 carbonic acid and 0·2020 water.

III. 0·2750 grm., formed by the action of caustic soda on rubian, gave 0·5605 carbonic acid and 0·1390 water.

IV. 0·4750 grm., obtained by means of ammonia, gave 0·9670 carbonic acid and 0·2295 water.

V. 0·2920 grm., obtained by means of caustic lime, gave 0·6000 carbonic acid and 0·1490 water.

These analyses give in 100 parts—

|          | I.    | II.   | III.  | IV.   | V.    |
|----------|-------|-------|-------|-------|-------|
| Carbon   | 55·93 | 55·76 | 55·58 | 55·52 | 56·04 |
| Hydrogen | 5·42  | 5·56  | 5·61  | 5·36  | 5·66  |
| Oxygen   | 38·65 | 38·68 | 38·81 | 39·12 | 38·30 |

corresponding to the formula $C^{52} H^{29} O^{27}$, as the following calculation shows :—

|          | Eqs. |     | Calculated. |
|----------|------|-----|-------------|
| Carbon   | 52   | 312 | 56·01       |
| Hydrogen | 29   | 29  | 5·20        |
| Oxygen   | 27   | 216 | 38·79       |
|          |      | 557 | 100·00      |

Rubianate of potash was prepared by dissolving the acid in as small a quantity of water as possible, adding an excess of carbonate of potash, allowing the salt to crystallize, collecting it on a filter, washing it with just sufficient water to remove the excess of alkali, and drying. On analysing it I obtained the following results :—

I. 0·3580 grm., dried in the water-bath and burnt with chromate of lead, gave 0·6830 carbonic acid and 0·1590 water.

0·5590 grm. gave 0·0780 sulphate of potash.

II. 0·4850 grm. of another preparation, dried *in vacuo*, gave 0·9250 carbonic acid and 0·2070 water.

0·8140 grm. gave 0·1150 sulphate of potash.

These numbers lead to the following composition :—

|          | Eqs. |       | Calculated. | I.     | II.    |
|----------|------|-------|-------------|--------|--------|
| Carbon   | 52   | 312   | 52·42       | 52·03  | 52·01  |
| Hydrogen | 28   | 28    | 4·70        | 4·93   | 4·74   |
| Oxygen   | 26   | 208   | 34·97       | 35·50  | 35·62  |
| Potash   | 1    | 47·2  | 7·91        | 7·54   | 7·63   |
|          |      | 595·2 | 100·00      | 100·00 | 100·00 |

The soda salt, prepared in the same way as the potash salt, was also analysed, but the analysis led to no satisfactory results.

A compound with baryta was obtained by dissolving the acid in water and adding ammonia and chloride of barium. The precipitate, which was bulky and of a fine crimson colour, was

placed on a filter and washed with water. During the washing its bulk diminished and the colour changed to red, probably in consequence of its losing part of its baryta. Analysis showed it to be a basic compound, in which the acid is to the base as $2:3$. The neutral baryta salt was obtained by adding chloride of barium to a watery solution of rubianate of potash, when a red precipitate fell, which was collected on a filter, washed with water, and dried *in vacuo.*

I. 0·6020 grm. of the latter compound, burnt with chromate of lead, gave 1·0800 carbonic acid and 0·2580 water.

0·5180 grm. gave 0·0925 sulphate of baryta.

II. 0·4440 grm. gave 0·7870 carbonic acid and 0·1900 water.

0·4890 grm. gave 0·0870 sulphate of baryta.

These numbers lead to the formula $C^{52} H^{28} O^{26} + BaO + HO$, as the following comparison of the experimental results with the theoretical composition shows:—

|  |  | Eqs. |  | Calculated. | I. | II. |
|---|---|---|---|---|---|---|
| Carbon | . | 52 | 312 | 49·24 | 48·92 | 48·34 |
| Hydrogen | . | 29 | 29 | 4·57 | 4·76 | 4·75 |
| Oxygen | . | 27 | 216 | 34·11 | 34·60 | 35·24 |
| Baryta | . | 1 | 76·6 | 12·08 | 11·72 | 11·67 |
|  |  |  | 633·6 | 100·00 | 100·00 | 100·00 |

A lead compound was prepared by adding to an alcoholic solution of the acid acetic acid and acetate of lead and then a little ammonia, taking care to leave an excess of rubianic acid in solution. The red precipitate was collected on a filter, washed with alcohol and then dried, at first *in vacuo,* and then for several hours in the water-bath. Its analysis gave the following results:—

0·5490 grm. gave 0·6760 carbonic acid and 0·1580 water.

0·6600 grm. gave 0·3760 sulphate of lead.

These numbers correspond in 100 parts to—

| Carbon | . | . | . | . | . | . | . | . | . | 33·58 |
|---|---|---|---|---|---|---|---|---|---|---|
| Hydrogen | . | . | . | . | . | . | . | . | . | 3·19 |
| Oxygen | . | . | . | . | . | . | . | . | . | 21·32 |
| Oxide of lead | . | . | . | . | . | . | . | . | 41·91 |

The most probable formula for this compound is $2C^{52} H^{28} O^{26} + 7PbO$, though the numbers found by experiment agree better with the formula $2C^{52} H^{27} O^{25} + 7PbO$, as will be seen from the following calculation:—

|  | Eqs. |  | Calculated. | Eqs. |  | Calculated. |
|---|---|---|---|---|---|---|
| Carbon . . | 104 | 624 | 33·22 | 104 | 624 | 33·55 |
| Hydrogen . | 56 | 56 | 2·98 | 54 | 54 | 2·90 |
| Oxygen . . | 52 | 416 | 22·17 | 50 | 400 | 21·51 |
| Oxide of lead | 7 | 781·9 | 41·63 | 7 | 781·9 | 42·04 |
|  |  | 1877·9 | 100·00 |  | 1859·9 | 100·00 |

Another specimen of the lead salt, prepared by precipitating an alcoholic solution of the acid with an alcoholic solution of basic acetate of lead, was found to have a composition agreeing tolerably well with the formula $C^{52} H^{28} O^{26} + 9PbO$. When this precipitate was redissolved in a mixture of alcohol and acetic acid, and a fresh precipitate was produced by means of a little ammonia, the latter was found to contain acid and oxide of lead in the proportion of 1 equiv. of the former to 6 equivs. of the latter. It appears, therefore, that these lead precipitates are by no means uniform in composition.

The silver salt was obtained by dissolving the acid in boiling water, adding a small quantity of ammonia and then nitrate of silver, when it fell in the form of a dark chocolate-coloured flocculent precipitate, which, when viewed under a lens, was sometimes seen to consist of small crystalline needles. The precipitate was collected on a filter and slightly washed with water. As it was found to dissolve in water with a reddish-brown colour, as soon as the excess of nitrate of silver was removed, the edulcoration was completed with alcohol, in which the salt is insoluble. After being dried *in vacuo*—

0·6300 grm. gave 0·1370 grm. chloride of silver, equivalent to 0·11076 or 17·58 per cent. of oxide of silver. The formula $C^{52} H^{28} O^{26} + AgO$ requires 17·47 per cent.

The alizarine and sugar obtained by the decomposition of rubianic acid with acids and alkalies, I found to have the usual composition, as the following analyses will show :—

I. 0·2190 grm. alizarine, obtained by the action of sulphuric acid on rubianic acid, dried at 100° C., yielded 0·5550 carbonic acid and 0·0850 water.

II. 0·3320 grm. alizarine, formed by the decomposition of rubianic acid with caustic soda, dried at 100° C., gave 0·8480 carbonic acid and 0·1195 water.

These numbers give in 100 parts—

|          | I.    | II.   |
|----------|-------|-------|
| Carbon   | 69·11 | 69·66 |
| Hydrogen | 4·31  | 4·00  |
| Oxygen   | 26·58 | 26·34 |

The sugar derived from the decomposition of rubianic acid with sulphuric acid was prepared for analysis by heating it, after the solution had been evaporated to a syrup, for some time at 100°C. until it became sufficiently brittle to be pulverized, when

0·3600 grm. yielded 0·5840 carbonic acid and 0·2060 water, corresponding with the following composition :—

P 2

|            |   |   |   | Eqs. |     | Calculated. | Found. |
|------------|---|---|---|------|-----|-------------|--------|
| Carbon     | . | . | . | 12   | 72  | 44·44       | 44·24  |
| Hydrogen.  | . | . | . | 10   | 10  | 6·17        | 6·35   |
| Oxygen     | . | . | . | 10   | 80  | 49·39       | 49·41  |
|            |   |   |   |      | 162 | 100·00      | 100·00 |

This specimen of sugar had therefore the same composition as that obtained by the action of erythrozym on rubian*.

The analysis of the acid, its compounds and products of decomposition just detailed, lead to the conclusion that its composition must be expressed by the formula $C^{52} H^{29} O^{27}$, and that it belongs to the class of the so-called glucosides, the copula contained in it being alizarine. Its decomposition, by means of strong acids and alkalies, into alizarine and sugar, is symbolized by the following equation :—

$$C^{52} H^{29} O^{27} + 5HO = 2C^{14} H^5 O^4 + 2C^{12} H^{12} O^{12}.$$
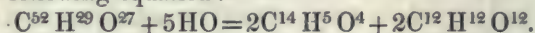
In order to remove all doubt concerning the true formula of this substance, I determined the quantities of alizarine which were obtained by decomposition of weighed quantities of the acid with sulphuric acid and with caustic soda.

1·7210 grm. rubianic acid, dried at 100° C., gave, when decomposed with sulphuric acid, 0·7310 grm. dry alizarine = 42·47 per cent.

1·2020 grm. rubianic acid yielded, when decomposed with pure caustic soda, on precipitation with sulphuric acid, 0·5430 grm. alizarine = 45·17 per cent.

Assuming the formula $C^{52} H^{29} O^{27}$ to be correct, 100 parts of acid should, according to theory, afford 43·44 of dry alizarine. If Strecker's formula for alizarine, $C^{20} H^6 O^6$, were the correct one, then the only possible formula for rubianic acid, though the calculated composition would then not agree very well with that found in my experiments, would be $C^{52} H^{27} O^{27}$, in which case the quantity of alizarine derived from 100 parts of acid would be 62·70. It will be seen, therefore, that the view which I have adopted of the constitution of the acid derives considerable support from these determinations.

A comparison of the composition of rubian and rubianic acid shows that the latter can only be derived from the former by means of oxidation. In order however to remove all doubt on this point, I made the following experiment. A solution of rubian was divided into two equal parts. One half was boiled to expel all the air it might contain, then mixed with a certain quantity of caustic soda, and immediately put into a bottle, which the liquid just sufficed to fill, and which was then closed

* Phil. Mag. for June 1853, p. 424.

air-tight. The other half was mixed with the same quantity of caustic soda and left exposed to the air. After twenty-one days both liquids were examined in the same manner. The soda was supersaturated with acetic acid, the liquid was again rendered alkaline with ammonia, and then chloride of barium was added, which gave in each case a red precipitate. This precipitate was treated as usual with sulphuric acid and carbonate of lead, and the filtered liquid was evaporated to dryness. From that part of the solution which had been enclosed in the bottle I obtained in this manner a quantity of a substance resembling rubian, which was probably rubidehydran, but not a trace of rubianic acid; whereas, by adding baryta water to the liquid filtered from the precipitate with chloride of barium, leaving the mixture to stand exposed to the air for some time, filtering and treating the substance left on the filter with sulphuric acid as usual, I obtained a small quantity of the acid. On the other hand, the precipitate with chloride of barium from the second half of the solution, which had been left in an open vessel, yielded at once 0·60 grm. of rubianic acid as well as a quantity of rubidehydran, but the liquid filtered from this precipitate, on being mixed with baryta water and treated as before, gave no more acid. Hence it follows that the presence of oxygen is as essential as that of alkalies to the formation of this acid.

As regards the manner in which the formation of rubianic acid takes place, we may suppose it to be effected in two ways. Assuming 1 equivalent of rubian to absorb 10 equivalents of oxygen, it may then yield 1 equivalent of rubianic acid, 4 equivalents of carbonic acid, and 5 of water, as the following equation will show :—

$$C^{56} H^{34} O^{30} + 10O = C^{52} H^{29} O^{27} + 4CO^2 + 5HO.$$
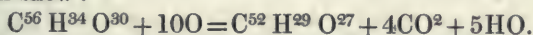
But since acetic acid is found among the products of decomposition, it is possible that this acid takes the place of carbonic acid, and the equation will then appear as follows :—

$$C^{56} H^{34} O^{30} + 2O = C^{52} H^{29} O^{27} + C^4 H^3 O^3 + 2HO.$$

It is a very remarkable circumstance, that a body like rubianic acid, which belongs to a class of a highly complex nature, and having in general a high atomic weight, should owe its formation to a process of oxidation. It is probably the first known instance in which the formation of a body of this class by means of oxidation has actually been observed.

Some years since Rochleder described a body obtained from madder to which he gave the name of *ruberythric acid**. This body, like rubianic acid, is decomposed by strong acids into alizarine and sugar, and the other properties mentioned by Roch-

* *Berichte der Wiener Academie*, April 1851.

leder are similar to those of rubianic acid. It is therefore very probable that the two acids are identical. But the description given by Rochleder of his acid is not sufficiently minute to enable me to come to a decision as to their identity, and the composition ascribed to it by him (C 54·48, H 5·16, O 40·36 per cent.) differs so much from that of rubianic acid as almost to lead one to believe that they are not the same. Until therefore the properties and composition of ruberythric acid have been more accurately studied, it will be impossible to arrive at any positive conclusion on this point, and for the present the two acids must be considered as distinct\*. Rochleder has ventured to express his conviction that rubian is nothing but impure ruberythric acid†. The experiments which I have here described must lead to the conclusion that such an opinion cannot for an instant be entertained. If rubianic acid and ruberythric acid are identical, then Rochleder has only committed the common error of mistaking a product for an educt.

*Rubidehydran.*—As obtained by the method above described, this body is not perfectly pure. It may be purified by redissolving it in water, evaporating the solution to a syrup, and then adding a quantity of alcohol, which precipitates a reddish-yellow glutinous substance mixed with sulphates of lime, magnesia, and soda. The alcoholic liquid having been allowed to clear, is decanted and evaporated to dryness, when it leaves a dark yellow or reddish-yellow, brittle, transparent residue like gum or varnish, which cannot be distinguished from rubian in appearance. It still gives, when burnt, a quantity of ash, consisting of sulphates of lime and magnesia, but I did not attempt to purify it any further, for fear of inducing a change in its composition. Rubidehydran resembles rubian exactly in most of its properties. It is not in the least deliquescent. If it shows any tendency to deliquesce on exposure to the air, this must be attributed to its containing some impurity. It has a strongly bitter taste. The watery solution is yellow, and when boiled, with the addition of sulphuric or muriatic acid, deposits yellow flocks, and after cooling appears almost colourless. Like rubian, it is also decomposed on boiling the watery solution with the addition of caustic potash or soda, the solution becoming purple and depositing yellow flocks on supersaturating the alkali with acid. It is not precipitated from its aqueous solution by any earthy or metallic

\* The easiest way of arriving at a conclusion would be by determining the amount of alizarine which ruberythric acid by its decomposition is capable of yielding. If Rochleder's formula $C^{72} H^{40} O^{40}$ be accepted, it should give 67·04 per cent. of alizarine. Variations of several per cent. in the amount of alizarine obtained would still allow a positive decision to be arrived at.

† *Ann. der Chem. und Pharm.* vol. lxxxii. p. 215.

salt, with the exception of basic acetate of lead, which produces a red precipitate, while the liquid becomes colourless. The alcohol solution, however, gives, with an alcoholic solution of neutral acetate of lead, a red precipitate, similar to the last, which is soluble in water. When exposed in a dry state to the action of heat, and to that of chlorine in its watery solution, rubidehydran behaves exactly like rubian. It may, however, be distinguished from rubian by its not yielding a trace of rubianic acid, when its watery solution is mixed with caustic soda or baryta and left exposed to the atmosphere for some time. Its products of decomposition with acids differ also from those of rubian. If a solution of the substance in water be boiled with the addition of sulphuric acid, yellow flocks are deposited, which consist principally of alizarine, with some rubiadine and a little verantine and rubiretine, but not a trace of rubianine, while the filtered liquid contains sugar. Rubidehydran yields therefore the same products with acids as rubian does with alkalies, which makes it very probable that rubian, when acted on by alkalies, is first changed, in part at least, into rubidehydran.

In calculating the composition of this substance from the analysis, I took it for granted that the lime and magnesia found in the ash are contained originally in it as sulphates, and therefore always treated the ash obtained in the first instance with sulphuric acid, in order to replace that portion of the sulphuric acid which might have been reduced during the process of ignition, and then made the corrections accordingly.

I. 0·4360 grm. rubidehydran, formed by the action of bicarbonate of baryta on rubian, dried at 100° C. and burnt with chromate of lead, gave 0·8005 carbonic acid and 0·2130 water.

0·2790 grm., on being incinerated, left 0·0150 grm. ash, which after being treated with sulphuric acid and heated again, weighed 0·0325 grm. = 11·64 per cent.

II. 0·4950 grm., formed by the action of lime-water on rubian, gave 0·9750 carbonic acid and 0·2320 water.

0·6220 grm. left 0·0200 grm. ash, which after treatment with sulphuric acid, weighed 0·0340 grm. = 5·46 per cent.

III. 0·4795 grm., obtained by means of caustic baryta, gave 0·9540 carbonic acid and 0·2320 water.

1·0785 grm. left 0·0245 grm. ash, which after treatment with sulphuric acid, weighed 0·0455 grm. = 4·21 per cent.

IV. 0·4990 grm., obtained by means of ammonia, gave 0·9780 carbonic acid and 0·2320 water.

0·7385 grm. left 0·0155 grm. ash, which after treatment with sulphuric acid, weighed 0·0395 grm. = 5·34 per cent.

After making the due corrections for the ash in the way just described, these numbers correspond in 100 parts to—

|  | I. | II. | III. | IV. |
|---|---|---|---|---|
| Carbon | 56·66 | 56·81 | 56·06 | 56·46 |
| Hydrogen | 6·13 | 5·50 | 5·54 | 5·45 |
| Oxygen | 37·21 | 37·69 | 38·40 | 38·09 |

leading to the formula $C^{56} H^{32} O^{28}$, as the following calculation shows:—

|  | Eqs. |  | Calculated. |
|---|---|---|---|
| Carbon | 56 | 336 | 56·75 |
| Hydrogen | 32 | 32 | 5·40 |
| Oxygen | 28 | 224 | 37·85 |
|  |  | 592 | 100·00 |

The lead compound was prepared by dissolving the substance in a small quantity of water, adding an alcoholic solution of acetate of lead, separating the red precipitate which was formed, by filtration, adding a little ammonia to the filtered liquid, taking care to leave an excess of rubidehydran in solution, filtering the precipitate and washing it with alcohol.

0·5705 grm. of this precipitate, dried in the water-bath, gave 0·6110 carbonic acid and 0·1465 water.

0·3180 grm., treated with sulphuric acid and heated, left 0·2200 grm. residue, which on being treated with hot water, yielded to the latter 0·0140 grm. sulphate of magnesia, leaving 0·2060 sulphate of lead.

These numbers lead to the following composition:—

|  | Eqs. |  | Calculated. | Found. |
|---|---|---|---|---|
| Carbon | 56 | 336 | 29·20 | 29·20 |
| Hydrogen | 32 | 32 | 2·78 | 2·85 |
| Oxygen | 28 | 224 | 19·48 | ,, |
| Oxide of lead | 5 | 558·5 | 48·54 | 47·66 |
|  |  | 1150·5 | 100·00 |  |

The deficiency in the oxide of lead in this analysis arose without doubt from the oxide being in part replaced by magnesia.

It appears therefore that rubidehydran differs from rubian merely by containing the elements of two equivalents of water less, and the origin of the different products of decomposition to which it gives rise may therefore be explained in the same manner as in the case of rubian.

Of its products of decomposition with acids I only obtained alizarine in quantities and of the degree of purity requisite for analysis. It had the usual composition of that substance, as the following analysis will show:—

0·2500 grm. of the crystallized substance lost, on being heated in the water-bath, 0·0460 water = 18·40 per cent.

0·2800 grm. of the dry substance gave 0·7110 carbonic acid and 0·1030 water, corresponding in 100 parts to—

|         |   |   |       |
|---------|---|---|-------|
| Carbon  | . | . | 69·25 |
| Hydrogen| . | . | 4·08  |
| Oxygen  | . | . | 26·67 |

*Rubihydran.*—This substance, like rubidehydran, bears a great resemblance to rubian, the body from which it is derived. It is obtained on evaporation of its solutions as an uncrystalline, transparent mass like gum, having a dark brownish-yellow colour and a bitter taste. When quite dry it is brittle and may be easily pulverized, but on exposure to the atmosphere at the ordinary temperature it rapidly attracts moisture and becomes soft, a property by which it may be distinguished from rubian and rubidehydran. On being heated in a tube it gives less crystalline sublimate than rubian does. Its watery solution, on being boiled with the addition of sulphuric or muriatic acid, becomes muddy, and slowly deposits a quantity of yellow flocks mixed with some brown resinous drops. The liquid must be boiled for a considerable time in order to effect the entire decomposition of the rubihydran contained in it and make it appear colourless. The flocks on being collected on a filter are found to consist chiefly of rubiretine, verantine, and rubiadine, with only a very small quantity of alizarine, while the filtered liquid contains sugar. The products of decomposition with acids are therefore the same as those of rubidehydran, the only difference being in the relative proportions of the products formed. It is not decomposed, when treated with boiling phosphoric, oxalic, tartaric or acetic acids. If a watery solution of rubihydran be boiled with caustic potash or soda, the colour of the solution, which was red on the first addition of alkali, changes to reddish- or yellowish-brown, but only a few purple flocks are deposited. This circumstance also serves to distinguish this substance from rubian and rubidehydran, the watery solutions of which, when mixed with caustic alkali and boiled, deposit an abundance of the purple compound of alizarine and alkali. Nevertheless the rubihydran is completely decomposed by the caustic alkali, for on adding an excess of acid to the liquid a quantity of yellow or light brown flocks are precipitated like those produced by the action of acids, while the liquid becomes almost colourless. When chlorine gas is passed through the watery solution, it produces exactly the same effects as in watery solutions of rubian or rubidehydran. It is the behaviour to chlorine, which more than any other reaction proves that these substances have a similar composition, and forms perhaps the most

characteristic mark of distinction between them and rubianic acid. Rubihydran is not precipitated from its aqueous solution by any reagent except basic acetate of lead, which produces a light brownish-red precipitate, which is paler and less bright than the precipitates produced by the same reagent in solutions of rubian or rubidehydran. The liquid filtered from this precipitate has still a light yellow colour, and ammonia produces in it a fresh precipitate of a pale pink colour, after which it appears colourless. Rubihydran is soluble in alcohol, but not very easily. From the manner in which it is prepared it may be inferred, that it is incapable of yielding rubianic acid by the influence of the same agents which lead to the formation of the latter from rubian. In order to obtain this substance in a perfectly dry state, it is necessary to heat it in the water-bath continuously for a day or two. In two analyses I obtained numbers corresponding respectively with the formulæ $C^{56} H^{45} O^{41}$ and $C^{56} H^{42} O^{38}$. I only succeeded once in obtaining it of the same composition, or as free from water as when in combination with oxide of lead. On this occasion it gave the following results:—

0·3940 grm. gave 0·7440 carbonic acid and 0·2130 water, numbers which lead to the following composition:—

|  | | Eqs. |  | Calculated. | Found. |
|---|---|---|---|---|---|
| Carbon | . . . | 56 | 336 | 51·29 | 51·50 |
| Hydrogen | . . | 39 | 39 | 5·95 | 6·00 |
| Oxygen | . . . | 35 | 280 | 42·76 | 42·50 |
|  |  |  | 655 | 100·00 | 100·00 |

On being burnt this specimen left no ash.

The lead compound was prepared by dissolving the substance in alcohol and then adding acetate of lead and ammonia, taking care to leave an excess of rubihydran in solution, filtering, washing the precipitate with alcohol and drying, at first *in vacuo*, and then for several hours in the water-bath.

I. 0·8740 grm. of this compound gave 0·6720 carbonic acid and 0·1970 water.

0·7270 grm. gave 0·5850 sulphate of lead.

II. 0·8860 grm. gave 0·6820 carbonic acid and 0·1930 water.

0·8465 grm. gave 0·6805 sulphate of lead.

In 100 parts:—

|  |  | I. | II. |
|---|---|---|---|
| Carbon | . . . . . | 20·96 | 20·99 |
| Hydrogen | . . . . | 2·50 | 2·42 |
| Oxygen | . . . . . | 17·34 | 17·44 |
| Oxide of lead | . . . | 59·20 | 59·15 |
|  |  | 100·00 | 100·00 |

The amount of oxide of lead here stands in no simple relation

to that of the other constituents. If the oxide of lead be deducted, then the rubihydran combined with it will have the following composition in 100 parts :—

|          |   |   |   |   | I.     | II.    |
| -------- | - | - | - | - | ------ | ------ |
| Carbon   | . | . | . | . | 51·37  | 51·38  |
| Hydrogen | . | . | . | . | 6·12   | 5·92   |
| Oxygen   | . | . | . | . | 42·51  | 42·70  |
|          |   |   |   |   | 100·00 | 100·00 |

It will be seen that this is exactly the composition of the substance itself according to the analysis given above, and it follows that the composition of rubihydran, both in a perfectly dry state and when in combination with oxide of lead, is expressed by the formula $C^{56} H^{39} O^{35}$. It differs therefore from rubian by containing the elements of 5 equivalents more of water. That it should yield the same products of decomposition as rubian and rubidehydran is therefore not at all extraordinary.

The rubiadine which is formed by the decomposition of rubihydran with acids may be obtained in a state of greater purity and with much greater facility from this, than from any other source. It is separated from the other products of decomposition in the manner I have described when treating of the action of alkalies on rubian. It is purified by dissolving it in boiling alcohol and adding to the boiling solution hydrated oxide of lead, when it crystallizes from the filtered solution in beautiful golden-yellow, glittering scales, some of which assume the form of regular four-sided tables, possessing all the properties of rubiadine*, but evidently freer from impurities than when obtained by the action of alkalies on rubian.

I. 0·3060 grm. of this substance, dried at 100° C., gave 0·7795 carbonic acid and 0·1380 water.

II. 0·2580 grm. of the same, recrystallized from boiling alcohol, gave 0·6600 carbonic acid and 0·1190 water.

From these numbers it may be inferred that its composition is as follows :—

|          |   |    |     | Calculated. | I.     | II.    |
| -------- | - | -- | --- | ----------- | ------ | ------ |
| Carbon   | . | 32 | 192 | 69·31       | 69·47  | 69·76  |
| Hydrogen | . | 13 | 13  | 4·69        | 5·01   | 5·12   |
| Oxygen   | . | 9  | 72  | 26·00       | 25·52  | 25·12  |
|          |   |    | 277 | 100·00      | 100·00 | 100·00 |

* In examining these crystals I discovered a property of rubiadine which I had not previously observed. If the ammoniacal solution, which is red, be mixed with chloride of barium, it deposits a quantity of dark brownish-red needle-shaped crystals, which are evidently the baryta compound of rubiadine, while the liquid loses almost all its colour. The rubiadine derived directly from rubian also yields these crystals.

The formula $C^{32} H^{13} O^9$, to which these analyses lead, differs from the one formerly given, $C^{32} H^{12} O^8$, by 1 equivalent of water. I prefer the former, since the substance employed in the last analyses was evidently purer than that used in any previous one. It will be seen also, that the formula just given is confirmed by an examination of the products of decomposition of rubian with chlorine.

[To be continued.]

XXIX. *The Doctrine of Parallel Lines considered in a new Method. By* JOHN STEVELLY, *LL.D., Professor of Natural Philosophy in Queen's University, Ireland.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,                                         Belfast, July 3, 1856.

HAVING been lately engaged teaching my youngest son the elements of Euclid, I was again induced to waste some hours on a subject on which in my schoolboy and college days I, in common I suppose with every schoolboy and collegian since the days of Euclid, had over and over again wearied myself in vain. On this occasion, however, the matter presented itself to my mind in a form which, to me at least, was quite new, and which seems to afford a solid foundation for the doctrine of parallel lines, and to place the subject in a form not perhaps unsuited to elementary instruction.

If you agree with me, and think it worth a place in your valuable Magazine, it is at your service.

I am, Gentlemen, with much respect,

Your obedient Servant,

JOHN STEVELLY.

*Definition.*

Let two straight lines in the same plane ($wx$ and $yz$) be said to be parallel when both stand at right angles to the same straight line (A, B).
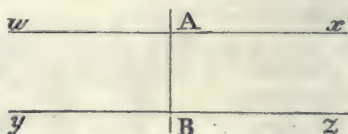


*Cor.* I. Hence two parallel lines, if produced indefinitely on either side, can never meet. (Otherwise a triangle would be formed with an external angle equal to an internal remote angle, as both are right. This would contradict the 16th of Euclid's first book.)

*Cor.* II. Hence equally distant points from A and B, taken along $wx$ and $yz$ on each side of AB, are equally distant from

each other. (This can be shown as a simple consequence of the
4th of Euclid's first book; but is manifest more simply by
supposing wAB*y* to be folded over upon AB; for, on account of
the right angles, wA, A*x* and *y*B, B*z* would coincide throughout
their entire length.)

### Proposition. *Lemma* I.

Let *wx* and *yz* be two parallel lines,
AB the line to which both are perpen-
dicular. Take D and E, equally distant
from A and B, on the same side of AB.
Bisect AB in C. Erect C*u* perpendicular
to AB. Then shall C*u* meet DE as in *m*,
a point which bisects DE; and C*u* shall
stand perpendicularly to DE.
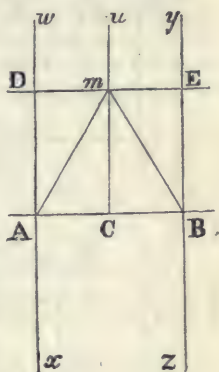
For C*u* is parallel to both *wx* and *yz*, and
therefore, if produced, can never meet either
(*Cor.* I. *Def.*). Therefore if produced, it
must cross DE between D and E. Suppose
at *m*, now join A*m* and B*m*. Then in the
triangles AC*m* and BC*m* we have the sides
AC and C*m* respectively equal to BC and C*m*, and the angles
contained by these sides (both right) equal, therefore (4th, first
book of Euclid) A*m* = B*m*, and the angles C*m*A = C*m*B, and
CA*m* = CB*m*; and since the angle DAC = EBC (both right),
hence *m*AD = *m*BE. Then in the triangles *m*AD and *m*BE we
have the sides DA and A*m* respectively equal to EB and B*m*,
and the angles contained by those sides also equal; therefore
(4th, Euclid's first book) D*m* = E*m*; and the angle D*m*A = E*m*A.
Therefore we have D*m*C = D*m*A + A*m*C = E*m*B + B*m*C = E*m*C,
and therefore *m*C perpendicular to DE, and bisects it. Q.E.D.

*Corollary.* Hence also angle EDA = DEB.

### Proposition. *Lemma* II.

Let AD and BE (fig. 1 or fig. 2) be any parallel and equally
long lines both perpendicular to AB. Join D and E. Then
shall the joining line stand perpendicularly both to AD and
to BE.

Take four points, *a*, *a'*, *b'*, and *b*, the two first near to and
equally distant on each side from A; the two latter, the same
distances on each side of B. Erect the four lines *ad*, *a'd'*, *b'e'*,
and *be*, each perpendicular to AB, and each equal in length to
AD or BE. Join *d* with *d'* by the straight line *dd'*, and *e'* with
*e* by the straight line *e'e*. These joining lines are each respect-
ively at right angles to AD and BE (produced if necessary), and
are bisected by them (*Lemma* I.). At D, draw the straight line

$w$D$x$ at right angles to AD, and at E the line $w'$E$x'$ perpendi-

Fig. 1.
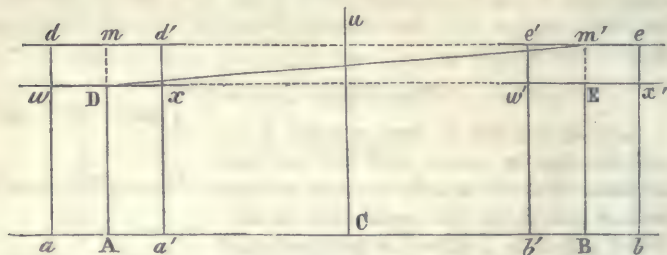


Fig. 2.
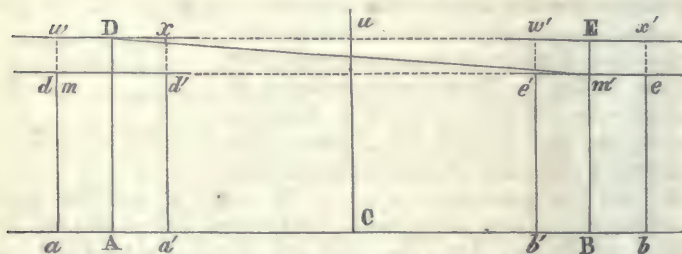


cular to BE.    Then if $d$ and $d'$ can be conceived not to be in the line $wx$, they must stand either equally above it, as in fig. 1, or equally below it, as in fig. 2 (as may be shown in a round-about manner by a mere application of the 4th of Euclid's first book, or), as may be (more) readily seen to be evident by con-ceiving $w$D$A$a$d$ to be folded over on the line AD ; when $w$D must coincide with $x$D, $a$A with $a'$A, and $ad$ with $a'd'$ ; therefore $d$ with $d'$ and the point where $ad$ meets $wx$, with the point where $a'd'$ meets it.    The same is true of the perpendicular line $w'$E$x'$, and the points $e'$ and $e$.    And since one line C$u$ stands perpen-dicular to AB at C, which at the same time bisects $ab$, AB, and $a'b'$, we may conceive the left-hand side of either figure to be so folded over C$u$ as to coincide in all similar lines and correspond-ing points with the right-hand side of that figure ; and therefore fig. 1 or fig. 2 must represent the circumstances, if the lines $wx$, $w'x'$, $dd'$, and $ee'$ can be supposed not to be coincident through-out : also on that supposition, $wx$, if produced indefinitely, could never meet $e'm'e$ ; nor could $dmd'$ ever meet $w'$E$x'$.    Hence, if we now join D with $m'$ by a straight line, no part of that join-ing line can in fig. 1 fall below D$x$, nor in fig. 2 above it (other-wise two straight lines would enclose a space).    Nor can any part of the same D$m'$ fall in fig. 1 above $e'm'$, nor in fig. 2 below

it (for a similar reason); and therefore D$m'$ must always lie between $wx$ and $dd'$, and between $w'x'$ and $ee'$, if they do not all coincide, let the points $a$, $a'$, $b'$, and $b$ be taken closer to A and B, or further from them. But since always $ad = AD = a'd' = b'e' = BE = be$, we may make $m'$ approach unlimitedly close to E by taking $b'B = Bb$ unlimitedly small; and similarly $m$ will approach unlimitedly to D, and $e'e$ to $w'x'$, and $d'd$ to $wx$; and therefore in the limit they all coincide; and D$m'$, which in the limit coincides with DE, is perpendicular to AD; and therefore DE (which never changes its position) is always perpendicular to AD; and similarly (or by corollary to Lemma I.) to BE.    Q.E.D.

*Cor.* I. Hence any line which is perpendicular to one of two parallel lines AD, must, if produced, meet the other, and be perpendicular to it also. For if a line be perpendicular at any point D to AD, take BE = AD and join DE; this joining line, being perpendicular to both, must coincide with the perpendicular to AD at D.

*Cor.* II. Hence all common perpendiculars to two parallel lines meet them at equal distances from A and B.

*Cor.* III. Hence all common perpendiculars to two parallel lines must be equal to one another throughout, and each must be equal to AB. For since DE (any joining line) and AB are both perpendicular to AD, they also are parallel; and since BE is perpendicular to both, AB = DE.
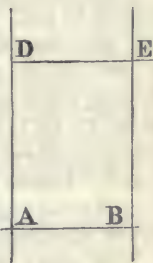
## *Prop.* 27 *of Euclid's First Book.*

If a line ($xy$) falling on two other lines ($wx$ and $yz$) make the alternate angles equal ($wxy = zyx$), these lines shall be parallel.
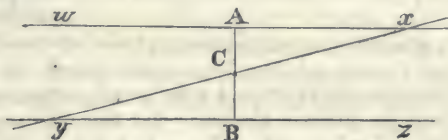
For bisect $xy$ in C, and let fall CA perpendicular on $wx$, and CB perpendicular on $yz$. Then in the triangles $x$AC and $y$BC, $x$C $= y$C, and the angle C$x$A $=$ C$y$B (hypothesis) and angle $x$AC $= y$BC (right); therefore (26th of Euclid's first book) the angle $x$CA $= y$CB. But $x$CA $+$ AC$y$ $=$ two right angles (13th of Euclid's first book) $=$ BC$y$ $+$ $y$CA. Therefore AC and CB are one continued line, and perpendicular to both $wx$ and $yz$, which are therefore parallel.    Q.E.D.

### Proposition 28 *of Euclid's First Book*

Follows at once, as in Euclid, from Prop. 27.

### *Prop.* 29 *of Euclid's First Book.*

Any line ($xy$) which falls upon two parallel lines ($wx$ and $yz$) makes (1) the alternate angles equal (as $wxy = zyx$); (2) any external angle equal to the internal, remote on the same side; and (3) the two internal, on the same side, together equal to two right angles. (1.) From $x$ let fall $xz$ perpendicular to $yz$; and from $y$, $yw$ perpendicular to $wx$. These lines are also respectively equal to each other, and perpendicular, the first to $wx$, the



latter to $yz$ (*Cor.* I. and III. *Lemma* II.). Hence in the two triangles, three sides of one equal respectively three sides of the other. Hence (8th of Euclid's first book) the angle $wxy = zyx$. The alternate angles are equal. From which the other two parts follow obviously as in Euclid.
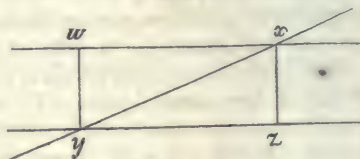
The other propositions follow as in Euclid, without any aid from the 12th axiom, which then becomes an easy deduction from the above, thus (as in the notes to Elrington's Euclid).

### *Euclid's* 12*th Axiom.*

If angles $xw\mathrm{A} + w\mathrm{A}z$ are together less than two right angles, the lines shall, if produced, at length meet on that side.
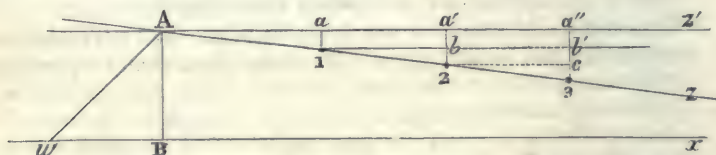


Draw $\mathrm{A}z'$ so as to make $xw\mathrm{A} + w\mathrm{A}z' =$ two right angles, $\mathrm{A}z'$ is parallel to $wx$ (as proved above). Let fall $\mathrm{AB}$ perpendicular to $wx$, and take $\mathrm{A}1 = 12 = 23 = $ &c. Let fall $1a$, $2a'$, $3a''$, &c. perpendiculars on $\mathrm{A}z'$, and draw $1b$, $2c$, &c. perpendicular to these.

It then is obvious from *Cor.* III. *Lemma* II., and from 26th of first book of Euclid, that $2a'$ is twice as long as $1a$, $3a''$ three times as long, and so on. Hence as we assume equal distances along $\mathrm{A}z$, the perpendiculars must at length become longer than BA, and therefore these points and also the line $\mathrm{A}z$ must at length pass to the other side of $wx$.   Q.E.D.

### XXX. *On* Scheutz's *Calculating Machine.*
### *By* G. B. AIRY, *Esq., Astronomer Royal.*

*To the Editors of the Philosophical Magazine and Journal.*

Royal Observatory, Greenwich,
August 25, 1856.

GENTLEMEN,

I REMARK that in your Supplementary Number for July
you have transcribed the Report of the Royal Society's Com-
mittee on M. Scheutz's Calculating Machine. I had the pleasure
of examining that machine shortly after its arrival in this country,
and I cannot refrain from expressing my general admiration of
the beauty of its arrangements, and my assurance that such a
machine is not likely to get out of order, and that it can be con-
structed at comparatively small expense. I am also impressed
with the ability and accuracy of the Committee's Report. But
I wish to guard myself from giving an opinion on the utility of
the machine; remarking only that, as I believe, the demand for
such machines has arisen on the side, not of computers, but of
mechanists.

Permit me, however, to point out to you a course of computa-
tions, of a most mechanical and monotonous kind, which is going
on frequently in every place of extensive and systematic calcu-
lations, and to which I think the attention of the constructors
of mechanical computing machines might advantageously be
directed.

An immense number of computations consist of parts follow-
ing each other in the following order:—

1st. Independent calculations are made (for instance, from
the lunar tables) for equal intervals (for instance, for every twelve
hours).

2nd. The differences of the computed numbers are taken,
probably to the fourth order, as a check on the accuracy of the
computed places.

3rd. Differences of the first and second order are formed, ap-
plicable to the interpolation of places for every hour.

4th. These differences are applied to form the places for every
hour.

The most tedious of these operations is the second (I assume
that the first can never be dispensed with).

This second operation appears to be well adapted to mechanical
action. Speaking (for the present) without mature consideration,
I should conceive, that, by reversing the order of the figures on
some of the wheels, and with some other instrumental changes,
and by feeding the proper wheels continually with new computed
numbers instead of new differences, the additive operations of
M. Scheutz's engine might be made subtractive; and the new

final result in every case might be a fourth difference instead of
a number formed by fourth differences.   At any rate, I conceive
that the object is within the reach of mechanical contrivance.

If this could be done, I really believe that a very important
benefit would be conferred on computers.

Of the other operations, the fourth would be effected by a very
simple difference-engine; possibly also the third might be con-
veniently done by mechanism, though it is sufficiently simple in
numerical calculation.

<div style="text-align:center">I am, Gentlemen,</div>

<div style="text-align:right">Your obedient Servant,<br>G. B. AIRY.</div>

## XXXI. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 149.]

Jan. 17, 1856.—Prof. William Allen Miller, M.D., V.P., in the
Chair.

THE following communication was read :—
"On some of the Metamorphoses of Naphthalamine."   By
A. W. Hofmann, Ph.D., F.R.S. &c.

The great facility with which some of the nitro-hydrocarbons can
be reduced by means of iron and acetic acid—the modification of
Zinin's process, lately proposed by M. Béchamp—enables us to
obtain the corresponding bases in larger quantity, and to examine
their derivatives more minutely.

One of the bases, for the preparation of which this process is par-
ticularly applicable, is naphthalidine, or as it is more appropriately
called, naphthalamine.   Mr. William H. Perkin is engaged in exa-
mining the deportment of this substance with chloride of cyanogen,
and the following is a summary of the results he has at present
obtained.

Fused naphthalamine, when submitted to the action of chloride of
cyanogen, absorbs this gas with great avidity, and is gradually con-
verted into a dark resinous mass.   This is the hydrochlorate of a
new base, which has received the name of menaphthalamine, in con-
sequence of the analogy of its origin with that of melaniline, derived
by a similar process from aniline.

Menaphthalamine is separated from the hydrochlorate by potassa,
washed and repeatedly recrystallized from alcohol.   It contains

$$C_{42} H_{17} N_3,$$
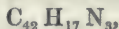
and is formed according to the equation

$$2C_{20} H_9 N + C_2 NCl = C_{42} H_{17} N_3, HCl.$$
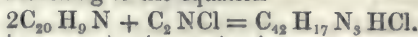
Naphthala-    Chloride of    Hydrochlorate of
mine.            cyanogen.      menaphthalamine.

Mr. Perkin has verified this formula of menaphthalamine by the

analysis of the hydrochlorate and of the platinum salt, which respectively contain

$$C_{42} H_{17} N_3, HCl \text{ and}$$
$$C_{42} H_{17} N_3, HCl, PtCl_2.$$
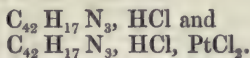
Among the various metamorphoses which menaphthalamine undergoes under the influence of agents, the deportment of this substance with cyanogen has especially engaged the attention of Mr. Perkin.

Menaphthalamine, like melaniline, absorbs two equivalents of cyanogen, and is converted into a slightly crystalline buff-coloured substance, which retains feebly basic properties.

The analysis of this body, which, from its composition, may be termed dicymenaphthalamine, has led to the formula

$$C_{46} H_{17} N_5 = C_{42} H_{17} N_3 + Cy_2.$$

Dicymenaphthalamine is insoluble in water, moderately soluble in alcohol and ether. It dissolves readily in acids. The latter solution, when precipitated by potassa, immediately after it has been made, yields unchanged dicymenaphthalamine; but if the solution be allowed to stand for a few moments, a yellow substance is precipitated, which is no longer a salt of dicymenaphthalamine.

The composition of this yellow body, which, in accordance with the terminology adopted in the aniline series, may be called menaphthoximide, is represented by the following formula—

$$C_{46} H_{15} N_3 O_4,$$
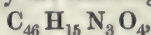
and its formation is illustrated by the equation—

$$\underbrace{C_{46} H_{17} N_5}_{\substack{\text{Dicymenaph-}\\\text{thalamine.}}} + 4HO + 2HCl = \underbrace{C_{46} H_{15} N_3 O_4}_{\substack{\text{Menaphthox-}\\\text{imide.}}} + 2NH_4 Cl.$$
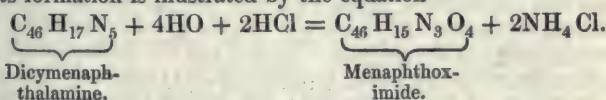
In fact, the mother-liquor of this substance contains a large amount of ammonia.

Menaphthoximide may be viewed as binoxalate of menaphthalamine *minus* 4 equivalents of water—

$$\underbrace{C_{42} H_{17} N_3 HC_2 O_4, HC_2 O_4}_{\text{Binoxalate of menaphthalamine.}} - 4HO = \underbrace{C_{46} H_{15} N_3 O_4}_{\text{Menaphthoximide.}};$$
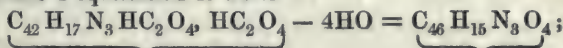
and this view is corroborated by the deportment of the substance with potassa, which reproduces menaphthalamine and oxalic acid.

From the preceding experiments, it is obvious that the deportment of naphthalamine with chloride of cyanogen is perfectly analogous to that of aniline. The subsequent metamorphoses of the newly-formed compound also exhibit the same analogy.

| Aniline series. | Naphthalamine series. |
|---|---|
| Aniline...... $C_{12} H_7 N$ | Naphthalamine .... $C_{20} H_9 N$ |
| Melaniline .. $C_{26} H_{13} N_3$ | Menaphthalamine .. $C_{42} H_{17} N_3$ |
| Dicymelaniline $C_{30} H_{13} N_5$ | Dicymenaphthalamine $C_{46} H_{17} N_5$ |
| Melanoximide $C_{30} H_{13} N_3 O_4$ | Menaphthoximide .. $C_{46} H_{17} N_3 O_4$ |
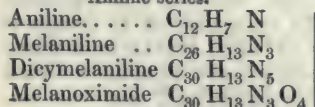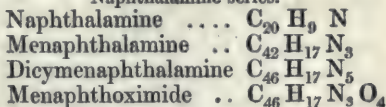
Results of much interest are to be expected from the examination of the products formed by the action of heat on menaphthoximide.

It is probable that this reaction would produce the naphthalamine term, corresponding to anilocyanic and cyanic acid.

$$\begin{array}{ll}
\text{Cyanic acid.} \ldots \ldots \ldots \ldots & C_2 \ H \ NO_2 \\
\text{Anilocyanic acid.} \ldots \ldots \ldots & C_{14} \ H_6 \ NO_2 \\
\text{Naphthocyanic acid} \ldots \ldots & C_{22} \ H_8 \ NO_2
\end{array}$$

Menaphthoximide, when heated, yields, in fact, a vapour of a most penetrating organic odour; but Mr. Perkin has not yet obtained sufficient material for a more minute examination of the body to which it belongs.

January 31.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

"Account of Pendulum Experiments undertaken in the Harton Colliery for the purpose of determining the Mean Density of the Earth." By G. B. Airy, Esq., Astronomer Royal.

In the first section of this paper, the author explains the reasons, founded on calculation, which appeared to make it probable that the comparison of gravity at the top and the bottom of a mine would give means of determining the earth's mean density with accuracy, perhaps superior to that obtained in the Schehallien or the Cavendish experiment; and which induced him first in the summer of 1826 (in concert with Dr. Whewell), and again in 1828 (with Dr. Whewell, Mr. Sheepshanks and others), to try the experiment in the Dolcoath mine near Camborne in Cornwall. These attempts were both frustrated by accidents having no connexion with the essential parts of the experiment. After a lapse of many years, he found that several circumstances (of which one was the general familiarity with the manipulation of the galvanic telegraph and the facility of applying it to the comparison of widely separated clocks) were very favourable to a repetition of the experiment; and having selected the Harton Colliery in the neighbourhood of South Shields as a fit place, in which two stations could be found in exactly the same vertical but at 1256 feet difference of height, and being assured of every assistance from the owners of the mine, he proceeded with the experiments in the months of September and October 1854.

The principal instruments employed were two detached pendulums on iron stands, the property of the Royal Society, which were most carefully repaired by Mr. Simms; graduated arcs, barometers, thermometers, &c.; two clocks, one the property of the Royal Society, which were fitted for this purpose with inclined gilded reflectors upon the pendulum bobs, intended to be illuminated by the light of lamps passing through holes in the side of the clockcases; galvanometer-needles attached to the clock-cases, with circuit-breakers; a galvanic battery at the upper station; a journeyman-clock at the upper station, fitted with an apparatus by which it completed the galvanic circuit at every $15^s$ of its own time; and two galvanic wires passing down the mine-shaft and forming a closed circuit through the battery, the journeyman-clock, and the two galvanometers.

The working party consisted of Mr. Dunkin (superintendent) and

Mr. Ellis from the Royal Observatory, Mr. Pogson from the Observatory of Oxford, Mr. Creswick from the Observatory of Cambridge, Mr. G. Rümker from the Observatory of Durham, and Mr. Simmonds from Mr. Carrington's Red Hill Observatory.

The plan of operations was this. Simultaneous observations of the two pendulums (one in the upper and the other in the lower station) were kept up *incessantly* during the whole working time (day and night) of one week; then the pendulums were interchanged and were observed in the same manner through another week; after this the pendulums were twice interchanged, but the two last series of observations were so much shortened that both were included in one week. Each pendulum had six swings of nearly four hours each, on every day of observation; and between the end of one swing and the beginning of the next, numerous galvanic signals were passed for the comparison of the clocks.

The second section gives the details (as far as space permits) of the comparisons of clocks by the galvanic signals. On examining the proportion of rates, it was found that there was distinctly a personal equation in the observation of the galvanic signals. Approximate values for the different observers were obtained, and the proportion of rates was corrected (where necessary) for these equations.

The third section describes the general system of observing the pendulums and reducing the observations. For ascertaining the time of a coincidence of the vibration of the detached pendulum with that of the clock pendulum, the mean of the times of the first disappearance and the last re-appearance was employed. Several coincidences were observed at the beginning of a swing and the mean was taken: and several were observed, and the mean taken, at the end of the swing. From these means, a mean interval of coincidences was obtained; from which the ratio of the actual rate of the detached pendulum to that of the clock pendulum was found. This requires several corrections.

The correction depending on the arc of vibration, with no data except the first and last arcs of vibration, and no assumption of a mathematical law for the intermediate arcs, is made to depend on the results of experimental observations on the numerical decrease of the arc by a peculiar process.

The corrections depending on the temperature and the atmospheric pressure are based mainly on Sabine's experiments.

The fourth section contains an abstract of the Pendulum Observations at the Upper Station, with the corrected logarithm of the rate of the detached pendulum on the clock pendulum for every swing; and the fifth section contains a similar abstract for the Lower Station.

The sixth section gives the computation of the logarithm of the rate of the lower detached pendulum upon the upper detached pendulum (for which the preceding sections have furnished the elements). Then is given in detail the investigation, by the Theory of Probabilities, of the formula for the best combination of the results of the different swings. The advantage of the method of incessant

observations with numerous comparisons of the clocks is pointed out. The formula is applied to the four series of observations; and the results of the first and third series agree very closely, and those of the second and fourth series agree very closely, showing that the pendulums had undergone no sensible change. By comparing the mean of the first and third series with the mean of the second and fourth, the proportion of pendulum rates at the upper and lower stations is obtained independently of the pendulums employed. The conclusion is that gravity below is greater than gravity above by $\frac{1}{19286}$th part, with an uncertainty of $\frac{1}{270}$th part of the excess; or that the acceleration of a seconds' pendulum below is $2^s\cdot24$ per day, with an uncertainty of less than $0^s\cdot01$. Reasons however are given for believing that the uncertainty is greater than this quantity.

The seventh section contains a description of the operation for measuring the depth of the mine. It then treats of the process to be employed for computing the proportion of gravity at the upper and lower stations (without reference to the experiments), on an assumed proportion of the density of the mine-rocks to the earth's mean density. It is shown that, supposing the upper surface of the ground about Harton to have the true spheroidal form, it is unnecessary to give any attention to the irregularities of the surface on distant parts of the earth. It is also shown that there is no reason to doubt the correctness of the law of decrease of the attraction of the earth's nucleus as depending on the elevation of the station, unless there be some serious irregularity in the arrangement or density of the matter immediately below Harton. Assuming this to be insensible, the theory of correction for the inequalities of ground in the neighbourhood of Harton is then considered. The elevation of the upper station is about 74 feet above high water; and as it appears from this that the depth of inequality can in no case amount to one-tenth of the depth of the lower station, it is easily found that the excess or defect of attraction will be computed with sufficient accuracy by supposing the excess or defect of matter to exist absolutely at the surface; in which case the effect on the upper station is nothing, and that on the lower station is easily computed. For depressions like that of the sea bounded (at least for the purposes of computation) by a straight line near the mine, but unlimited in the other direction, a simple formula is found.

For the application of these theorems it was necessary to have a map giving the elevations of the ground at various points. By instruction of the Mayor and Corporation of South Shields, the Corporation Surveyor, Christopher Thompson, Esq., prepared such a map. In the use of it, it was found convenient to adopt as unit of linear measure the depth of the mine. A line at the distance of ten depths very nearly touches the cliffs of Tynemouth, of Frenchman's Point, and of some points further to the south-east. The land generally is divided into squares whose sides are one depth each, and these are grouped as appears convenient for representing approximately the form of the ground by compartments each of a uni-

form elevation through its extent. The principal requirements are, besides taking account of the depression of the sea beyond the ten-depth line, to estimate the effect of the curvature of the coast towards the mouth of the Wear, to compute the effect of the hollow of Jarrow Slake, and generally to make proper allowance for the absence of matter in the valley of the Tyne. There are also some small elevations to be considered. The general result is, that the attraction of the regular shell of matter is to be diminished by about $\frac{1}{280}$th part.

Putting $D$ for the mean density of the earth, $d$ for that of the shell, the fraction $\dfrac{\text{Gravity below}}{\text{Gravity above}}$ is computed to be $1\cdot00012032 - 0\cdot00017984 \times \dfrac{d}{D}$. The pendulum experiments give $1\cdot00005185$.

The comparison of these gives $\dfrac{D}{d} = 2\cdot6266$.

The eighth section contains a detailed account of the strata passed through in sinking the Harton shaft, and the specific gravities of many of the beds as determined by Professor W. H. Miller. The result for the mean specific gravity is $2\cdot50$. Substituting this in the equation given by the pendulum experiments, the mean specific gravity of the earth is found to be $6\cdot566$. Adverting to the excess of this number above those given by the Schehallien and the Torsion-rod experiments, the author remarks that it is very difficult to assign the causes or the measures of error in either of the experiments, but expresses his belief that the result of the present experiment may compete on at least equal terms with the others.

Feb. 14.—Dr. W. A. Miller, V.P., in the Chair.

The following communication was read:—

"On Periodical Laws discoverable in the Mean Effects of the larger Magnetic Disturbances."—No. III.    By Colonel Edward Sabine, R.A., D.C.L., Treas. and V.P.R.S.

In two previous papers bearing the same title as the present (Phil. Trans. 1851, Art. V., and 1852, Art. VIII.*), the author showed, from the hourly observations of the magnetic Declination at Toronto and Hobarton, that the magnetic disturbances of large amount, and apparently irregular occurrence, commonly called *magnetic storms*, are found, when studied in their *mean* effects, to be governed by periodical laws of systematic order and regularity, and to exhibit periods whose duration is, respectively, 1, a solar day; 2, a solar year; and 3, a period of about ten of our solar years, corresponding both in duration and in the epochs of maximum and minimum variation, to the approximately decennial period discovered by Schwabe in the phænomena of the solar spots. In the present paper the author communicates the results of a similar investigation into the laws of the disturbances of the two other magnetic elements at Toronto, namely, the Inclination and the Total Force, derived from the hourly observations of the horizontal and vertical Forces during the five years from July 1843 to June 1848; affording, as he states,

* See also Phil. Mag. S. 4. vol. i. p. 498, and vol. iv. p. 232.

a full confirmation of the existence of periodical laws regulating the disturbances of the Inclination and Total Force corresponding to those which he had previously deduced from the disturbances of the other magnetic Element, viz. the Declination.

Feb. 21.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Bromide of Titanium." By F. B. Duppa, Esq.

A comparison of the boiling-points of corresponding chlorine and bromine compounds, led Prof. Kopp to the interesting discovery, that on the average their boiling-points rise 32° C. for every equivalent of bromine which is substituted for an equivalent of chlorine.

|  | Boiling-point. | Difference. |
|---|---|---|
| Chloride of ethyle, $C_4 H_5 Cl$ .... | 11° C. | } 30. |
| Bromide of ethyle, $C_4 H_5 Br$ .... | 41° C. | |
| Dichlorinated ethylene, $C_4 H_4 Cl_2$ | 67° C. | } $66 = 2 \times 33.$ |
| Dibrominated ethylene, $C_4 H_4 Br_2$ | 133°·6 C. | |
| Terchloride of phosphorus, $P Cl_3$ . | 78° C. | } $97 = 3 \times 32\frac{1}{3}.$ |
| Terbromide of phosphorus, $P Br_3$ . | 175° C. | |

If this difference be constant for all chlorine and bromine compounds, it becomes obvious that very important inferences in respect to the atomic constitution of these substances may be derived from the determination of their boiling-points. This result has, in fact, been happily applied by Prof. Kopp, as a criterion to determine the equivalent of silicium, a matter of such uncertainty as to have led to the admission of not less than three formulæ for silica—

$$SiO$$
$$SiO_2$$
$$SiO_3.$$

From the difference between the boiling-points of chloride (59° C.) and that of bromide (153° C.)—a difference which amounts to $94 = 3 \times 31\frac{1}{3}$—Kopp derives the formulæ

$$SiCl_3 \text{ and } SiBr_3,$$

as representing the atomic constitution of the chloride and the bromide of silicium, and he accordingly fixes the equivalent of silicium at 21·3.

In order, however, to prove the general validity of Kopp's observations, it was necessary to re-examine the boiling-points of corresponding chlorine and bromine compounds which exhibited discrepancies, and to extend the inquiry to as great a number of new compounds as possible.

⸱ The bromine-compound of titanium was unknown. Mr. Duppa has produced this substance by passing a current of bromine over an intimate mixture of pure titanic acid and carbon. The reaction takes place at a bright red heat, and furnishes a brown liquid, which solidifies in the receiver to a crystalline mass. Distilled with an excess of mercury, which removes any free bromine that may be present, the bromide of titanium presents itself as an amber-yellow compound, exhibiting a magnificent crystalline structure; it attracts moisture with the greatest avidity, and is converted into titanic and hydrobromic acids. Bromide of titanium has a specific gravity of

2·6. The fusing-point was found, 39°. The boiling-point was examined by Mr. Duppa with a considerable quantity of substance, the purity of which had been ascertained by analysis. It was observed to be 230° C. The boiling-point of the chloride of titanium, as observed by Dumas, and confirmed by Mr. Duppa, is 135°. The difference, $230 - 135 = 95 = 3 \times 31\frac{1}{3}$, is exactly the same as that observed between the boiling-points of chloride and bromide of silicium.

This observation furnishes an additional support to the analogy of silicium and titanium, while it points unequivocally to the formulæ

$$TiCl_3 \text{ and } TiBr_3,$$

as representing the atomic constitution of these two compounds.

Titanic acid, hitherto universally represented as a binoxide $TiO_2$, would then assume the formula

$$TiO_3$$

in perfect analogy with that of silicic acid.

The equivalent of titanium would then be changed from 24·29, the number at present adopted, to 36·39. The protoxide of titanium would in this case become a sesquioxide, and the compound hitherto viewed as sesquioxide would have to be considered as an intermediate oxide—as a combination of the sesquioxide with the teroxide, in fact, as a bititanate of sesquioxide of titanium.

*Formulæ of the Titanium Compounds.*

| Old Notation. | | New Notation. |
|---|---|---|
| $Ti = 24·29$ | | $Ti = 36·39.$ |
| $TiO$ | First oxide | $Ti_2O_3$ |
| $Ti_2O_3$ | Second oxide | $Ti_4O_9 = Ti_2O_3, 2TiO_3$ |
| $TiO_2$ | Acid | $TiO_3$ |
| $TiCl_2$ | Chloride | $TiCl_3$ |
| $TiBr_2$ | Bromide | $TiBr_3.$ |

It is scarcely necessary to observe, that an alteration of the equivalent of titanium on the ground of the difference of the two boiling-points, would be hazardous, if not supported by additional experimental evidence, and that further researches on the series of titanium are required in order to establish whether the proposed alteration actually affords a simpler expression for the combining relations of this remarkable element*.

---

## XXXII. *Intelligence and Miscellaneous Articles.*

### ABSORPTION OF HEAT BY DECOMPOSITION.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,                                   Parsonstown, August 1856.

IN reference to Mr. Joule's continued wish to be thought the originator of the discovery that chemical decomposition absorbs heat, allow me to say that I am sorry to prolong a discussion which involves chiefly personal interests. I would not again urge my claim

* Some observations on this paper by Dr. Kopp will be found at p. 190.—EDS.

to the discovery which I am sure the scientific world will allow me when the matter is properly weighed, were it not for the statement Mr. Joule makes at the commencement of his reply in your Magazine for the present month. He says I mis-state the results he arrived at, and that I endeavour to support my claim to an original discovery by setting one part of his papers against another.

To neither of these accusations can I silently submit. As to the first, my statement is, that Mr. Joule passed a galvanic current through a wire and found a certain quantity of heat generated, and that he then passed it through an electrolyte and did not find the same amount of heat produced for an equal resistance. There is no mis-statement in this; and Mr. Joule in his reply shows it so clearly, that I have only to quote his own words to prove it. He says (p. 155 of the Number of this Magazine for this month), " Suppose, first, we take a voltaic battery of 20 iron-zinc pairs, and connect its terminations by a metallic wire. After a certain interval of time, we find 100 atoms or chemical equivalents of zinc dissolved in each cell of the battery. Then the entire amount of heat evolved will be expressed by $100 \times 20 = 2000$. Suppose, secondly, we employ this battery to decompose water; the virtual intensity of the entire battery circuit will then be $20 - 3\frac{1}{3} = 16\frac{2}{3}$ (see p. 272 of the above-mentioned memoir); and when 100 atoms of zinc in each cell of the battery have been dissolved, or what comes to the same thing, when 100 atoms of hydrogen have been liberated in the decomposing cell, the heat evolved by the entire circuit will be expressed by $100 \times 16\frac{2}{3} = 1666$. The difference between 2000 and 1666, or 334, represents of course the heat taken up in effecting the separation of the 100 atoms of hydrogen from 100 atoms of oxygen, and liberating them in a gaseous state." Now here, when the metallic wire is used, there are 2000 units of heat produced; but when the electrolyte is used there are only 1666. The same amount of heat is not evolved in the first and second case; such is the result I stated he arrived at, and such he says is a mis-statement. I do not understand why he makes the assertion. There is a difference, 334, between the result of the two experiments; and the cause of this difference, or rather the first determination of the cause, is just the question at issue between Mr. Joule and me. He says, "it represents of course the heat taken up by the separation of the oxygen and hydrogen." Of course it does; but when did Mr. Joule *first* tell the world of this simple fact? Nine months after I published it in October 1851 in this Magazine. In 1841 Mr. Joule published a paper in the Philosophical Magazine proving, that the heat developed by a galvanic current is proportional to its intensity. When the current passed through an electrolyte, Mr. Joule thought part of the intensity was used up in causing electrolysis, and that the remainder only was effective in producing heat; that for this reason (to use his own example already quoted), instead of 2000 units there were only 1666 units of heat produced; ascribing the difference 334, not to the separation of the oxygen and hydrogen, as he does now, but to the diminished intensity of the battery. In this supposition, however, he was quite in error. There is no loss of intensity or

quantity of galvanic current except what is caused by the bad conducting power of the electrolyte, and the same amount of heat is evolved in a solid and liquid conductor by the same electromotive power if it acts against the same resistance; but part of the heat so produced is *absorbed* by the decomposition or separation of the elements. It is not that the galvanic current is diminished by effecting electrolysis, otherwise than it is when it passes through a badly conducting solid, for the whole current is effective, and the whole amount of heat due to it is produced; but simply that part of that heat is rendered latent by the decomposition.

If a compound body is decomposed without using the battery at all, and that heat is absorbed, or does not make its appearance, that loss of heat cannot be ascribed to diminished intensity of galvanic current. If, for instance, potassium be thrown on water, the oxygen unites with the potassium and the hydrogen escapes, the water being decomposed. Now does the combination of the potassium and oxygen produce as much heat in this case as if the elements were presented to each other in a free state? Decidedly it does; but the decomposition of the water absorbs part of it and renders it latent: and this quantity so rendered latent is the same as the combination of the oxygen and hydrogen would themselves produce. This is the principle of my discovery. Mr. Joule, in the paper he refers to, published in this Magazine in 1841, does not at any time even hint at an absorption of heat by decomposition, or by anything else. He merely says that decomposition prevents part of the current being effective in producing heat; so far from saying heat is absorbed, he says it is not evolved at all. His paper would never lead to the supposition that decomposition produced by superior chemical affinity would cause absorption of heat. That is my discovery, and therefore I claim to have laid the foundation of all the thermo-chemical researches which take decomposition into account. None were carried on before the publication of my paper in 1851.

Mr. Joule says, that, independently of his paper in 1841, he should have priority because he sent a paper to the French Academy in 1846. I do not know exactly the etiquette followed in such cases; all I know is, that I worked very hard for a long time to find whether certain theoretical conclusions were experimentally correct; and finding that they were so, I condensed the labour of months into a short paper and published it, believing it might be of use. This paper having been then made the foundation of other researches, Mr. Joule interferes and says he knew all about it long before I did. I have no doubt of what he states; but that knowledge not having been published, did me no good, and perhaps it might still be a dead letter if I had not brought it out by having worked myself. I therefore think if there is any credit to be attached to the discovery, that I deserve not only my own but his. But as to this paper sent to the French Academy, has it been published exactly as it is in their hands? Perhaps the absorption of heat might not be shown in it at all. I say this, because Mr. Joule brings forward his paper of 1841 to prove that he was aware of this truth: and really if I were wishing to show proof of his not having at that time known anything

about it I would point to that paper. In the first place, he does not speak of it; and in the second place, when about to calculate the amount of heat produced in the cell of a voltaic pair composed of zinc and silver, he says (p. 266 of the paper he refers to in vol. xix. Phil. Mag. 1841), " Before I proceed to give an account of some experiments on heat evolved in cells of voltaic pairs, it is important to observe that every kind of action not essentially electrolytic must be eliminated," &c. He then, in calculating the heat produced, makes allowance for the combination of the oxide of zinc and sulphuric acid, but does not at all mention the absorption of heat produced by decomposition of the water, which is of greater amount, in the opposite direction.

The several data to be taken into account in calculating the amount of heat produced by the solution of zinc in sulphuric acid are,—the oxidation of the zinc, the combination of the oxide of zinc and acid, and the absorption of heat by decomposition. This last Mr. Joule does not allude to at all; and when it is considered that he sets out with the determination to eliminate all actions which might interfere with the proper amount of heat being arrived at, it must be concluded that at that time he did not know that heat was absorbed by decomposition.

Now as to Mr. Joule's second charge, that I set up one part of his papers against another, I can only say I merely quote his own words to show that opinions he once held he afterwards altered, and that I endeavoured to show his first opinions were correct. If any contradiction appears, he should rather blame himself than me. He says I *erroneously* suppose he contradicts himself. The matter stands thus :—Mr. Joule proved experimentally (vol. xx. Phil. Mag. 1842) that the amount of heat produced by oxidation might be taken as a measure of the intensity of the attraction of affinity between the combining bodies; but in a subsequent paper (Phil. Mag. vol. xxiii. p. 442) he says that " it is not precisely the attraction of affinity, &c. which determines the intensity of the current and consequently the amount of heat evolved." This is the contradiction he says I erroneously imagine. Without being aware that these papers had been published, I had myself proved experimentally the first proposition; and when Mr. Joule claimed it, I at once, on referring to his first publication, admitted his claim; but added, that if the proposition were true I should still consider I established it, as that although he first held, he afterwards rejected it.

In endeavouring to establish a claim to a disputed discovery, it is impossible not to make reference to previous papers written by the disputing parties. I have done so without having the least wish to take any undue advantage of anything which has been written, or which has not, but which ought to have been written if the truth had been recognized. I trust that nothing I have said can make it appear that I have not the highest respect for such a philosopher as Mr. Joule, of whose high talents and achievements in science his country ought to feel proud; or that I am not deeply grateful for

the many discoveries, and great and important truths, he has so laboriously proved and so freely given to the world.

I have the honour to be, Gentlemen,

Yours very respectfully,

Thomas Woods, M.D.

OBSERVATIONS AND EXPERIMENTS UPON THE EMPLOYMENT OF IODIDE OF POTASSIUM AS A REAGENT FOR OZONE. BY S. CLOËZ.

The experiments of Marignac, Fremy, and Becquerel have done most to clear up the question of the nature of ozone; they prove completely the possibility of imparting to chemically pure oxygen all the properties of this mysterious substance.

Iodide of potassium being one of those substances upon which ozone is capable of acting, paper soaked in a solution of starch containing 0·002 of its weight of this iodide has been proposed, under the name of the *ozonometric reagent*, not only to indicate the presence of ozone, but also to measure the quantity contained in the air. If the coloration of this paper could only be produced by ozonized oxygen, its employment would leave nothing to be desired. But this is not the case; acid vapours act upon iodide of potassium in the same manner as active oxygen; the essential oils exhaled by plants have the same action, and prove that moist air, under the influence of the direct light of the sun, colours the reagent, although we cannot suppose the air to be ozonized.

The experiments upon nitrification which the author is carrying on simultaneously at the Museum of Natural History and the Polytechnic School, have led him to investigate what part the ozonized oxygen said to exist in the air may play in the formation of nitric acid. After a great number of trials he has not yet succeeded in establishing with certainty the part taken by ozone in the phænomenon of nitrification, effected in the absence of azotized or ammoniacal substances, but his experiments have convinced him that the numerous attempts to prove the presence of ozone in the air and measure its quantity by means of iodized paper, are not of the least value.

It is an admitted fact that the coloration of the paper takes place every day in the country, and in places where there are many trees and an active vegetation, whilst many observations prove that in the most populous parts of towns the paper is very rarely and very slightly coloured. The author has found that in the Garden of Plants the iodized paper is constantly coloured by exposure, especially in the neighbourhood of resiniferous trees, and frequently in a very short time, but that at the Polytechnic School, in the vicinity of which there are few trees, but a dense population living in unhealthy houses, the paper exposed daily to the air under the same conditions as that at the Museum has not been coloured once in six months. M. Cahours has observed a similar contrast between his garden at Neuilly and the Hôtel des Monnaies in Paris.

Resiniferous trees, aromatic plants, and all the parts of vegetables which contain volatile oils, act much more strongly than inodorous

plants upon iodized paper in their vicinity. By passing moist air through a tubulated bell-glass covering the plants to be experimented on, and exposing the iodized paper to the air at its exit from the bell, it will be seen that whenever the plant is capable of producing odorous volatile substances, coloration takes place ; in the opposite case the paper remains white.

From some experiments recently published, it would appear that the oxygen disengaged by the green parts of plants under the influence of light is in the same state as the gas produced by the electrolytic decomposition of water, or the nascent oxygen prepared in the cold by the action of sulphuric acid upon binoxide of barium. The author has found that this oxygen has no effect upon iodized paper. He placed some aquatic plants in a bottle filled with rain-water containing about half its volume of carbonic acid, exposed the apparatus to the sun, and collected the gas under a test tube filled with water ; the gas produced no coloration of iodized paper by contact for six hours. As it might be objected that the gas had lost its oxidizing power during the short space of time occupied in collecting it, he adapted to the neck of the bottle containing the plants, a glass tube of 3 decimetres in length, covering its lower half with black paper, and introducing a strip of test paper both into this and into the portion left exposed to the light. The apparatus was exposed for two days to the sun ; 2·25 litres of moist gas were evolved, the whole of which passed over the iodized paper, of which the strip protected from the light was unchanged, whilst the other was strongly coloured. This is the constant effect of the action of light upon iodized paper in the presence of moist oxygen.

It cannot be admitted that, as advanced by Schönbein, and lately repeated by Scoutetten, light ozonizes the air, for although the active modification of oxygen is not permanent, it may be kept for several hours ; and if light possessed the property attributed to it, moist air exposed to the sun, and removed for a short time from the action of the solar rays, ought to act upon iodized paper in the manner of ozone ; but this is never the case. However long the air may be exposed to the sun, it is never ozonized.—*Comptes Rendus*, July 7, 1856, p. 38.

* * *

## ON THE PRESENCE OF MERCURY IN THE NATIVE ARGENTIFEROUS COPPER OF LAKE SUPERIOR. BY M. HAUTEFEUILLE.

The author received 200 kilogrammes of this mineral to ascertain its value. Its gangue consisted principally of calcareous spar, which was got rid of by dilute muriatic acid ; its weight was 50·476 kilogrms. The copper was covered with larger and smaller tufts of metallic silver, which were removed by the graver. Fragments of copper were selected which did not appear to have had any silver on the surface, and cut into the centre ; the author found in them 0·002 of silver ; the whole gave 0·008 of silver when melted. The total weight of copper was 138·560 kilogrms.

The silver removed by the graver was assayed by the humid way and estimated at 0·993, but the assay presented the following phænomena:—1, the liquid was scarcely limpid ; 2, the chloride of silver

did not acquire colour when exposed to a bright light, hence the author concluded that mercury was present. To settle this point, the author boiled the chloride of silver with a plate of gold, a little tin, and muriatic acid; the gold plate was amalgamated. He then treated 150 grms. of the silver separated by the graver by M. Levol's process, seeking for the mercury in the liquid deprived of silver and containing the double acetate of ammonia and mercury. By means of protochloride of tin he obtained of metallic mercury $0·0035 = 0·038$ kilogrms., the silver being 10·906 kilogrms. Thus this copper contains,—

|  | Kilogrms. | Per 1000. |
| --- | --- | --- |
| Copper ............ | 138·560 | 0·69280 |
| Silver ............. | 10·906 | 0·05453 |
| Mercury........... | 0·038 | 0·00019 |
| Gangue ........... | 50·496 | 0·25248 |
|  | 200·000 |  |

—*Comptes Rendus*, July 21, 1856, p. 166.

## METEOROLOGICAL OBSERVATIONS FOR JULY 1856.

*Chiswick.*—July 1. Cloudless, with very dry air. 2. Dry haze: very fine. 3. Clear and very fine. 4. Fine: overcast: slight rain. 5. Fine: overcast: very fine. 6. Very fine: lightning, with rain at night. 7. Densely clouded: rain. 8. Very heavy rain, cold and boisterous. 9. Fine. 10. Very fine. 11. Cloudy: slight drizzle: fine. 12. Overcast. 13. Very fine: overcast: cloudy. 14. Very fine: overcast. 15. Cloudy and fine: lightning, with rain at night. 16. Rain: heavy clouds and showers: very fine. 17. Very fine. 18. Overcast: drizzly: very fine. 19. Fine: overcast. 20. Slight drizzle: overcast: rain. 21. Very fine. 22. Sultry. 23. Slight fog: very hot: cloudy: lightning. 24. Shower: very hot. 25, 26. Exceedingly fine. 27. Overcast: rain. 28. Cloudy: very fine: hazy. 29. Slight fog: very fine. 30. Uniform haze: very fine. 31. Slight fog: very sultry: clear and fine.

Mean temperature of the month ................................. 61°·20
Mean temperature of July 1855 ................................. 62 ·99
Mean temperature of July for the last thirty years ......... 63 ·11
Average amount of rain in July ................................. 2·558 inches.

*Boston.*—July 1—4. Fine. 5. Cloudy. 6. Cloudy: rain P.M. 7. Cloudy: rain A.M. and P.M. 8. Fine: rain A.M. and P.M. 9. Fine: rain P.M. 10. Fine. 11. Cloudy. 12. Cloudy: rain A.M. and P.M. 13. Cloudy. 14. Fine. 15. Cloudy. 16. Cloudy: rain A.M. and P.M. 17. Fine. 18. Rain A.M. and P.M. 19. Fine. 20. Cloudy. 21, 22. Fine. 23, 24. Cloudy. 25. Fine: rain P.M. 26. Cloudy. 27. Cloudy: rain A.M. 28—31. Fine.

*Sandwick Manse, Orkney.*—July 1. Bright A.M.: clear P.M. 2. Clear A.M. and P.M. 3. Clear A.M.: showers P.M. 4. Drizzle A.M. and P.M. 5. Cloudy A.M.: drizzle, showers P.M. 6. Showers A.M.: drizzle, showers P.M. 7. Rain A.M.: bright P.M. 8. Drizzle, showers A.M. and P.M. 9. Drizzle, showers A.M.: cloudy P.M. 10. Cloudy A.M. and P.M. 11. Cloudy A.M.: rain P.M. 12. Bright A.M.: bright, fine, thunder P.M. 13. Bright A.M.: cloudy, fine P.M. 14. Clear, fine A.M.: cloudy, rain P.M. 15. Cloudy A.M. and P.M. 16. Drizzle A.M.: rain P.M. 17. Clear A.M. and P.M. 18. Rain A.M.: showers P.M. 19, 20. Drizzle A.M.: damp P.M. 21. Bright, fine A.M. and P.M. 22. Clear, fine A.M.: cloudy, fine P.M. 23. Rain A.M.: cloudy, fine P.M. 24, 25. Bright A.M.: bright, fine P.M. 26. Clear A.M.: showers P.M. 27. Bright A.M.: showers P.M. 28. Showers A.M.: cloudy, fine P.M. 29. Clear, fine A.M.: rain P.M. 30. Bright A.M.: hazy P.M. 31. Cloudy, fine A.M.: rain P.M.

Mean temperature of July for previous twenty-nine years ... 55°·22
Mean temperature of this month ................................. 54 ·77
Mean temperature of July 1855 ................................. 59 ·19
Average quantity of rain in July for previous sixteen years ... 2·45 inches.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.*

| Days of Month. 1856. July. | Barometer Chiswick Max. | Chiswick Min. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 8¾ p.m. | Thermometer Chiswick Max. | Chiswick Min. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 8¾ p.m. | Wind Chiswick 1 p.m. | Boston | Orkney, Sandwick | Rain Chiswick | Boston | Orkney, Sandwick |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. | 30.202 | 30.178 | 29.74 | 30.17 | 30.17 | 71 | 44 | 53 | 49 | 49½ | ne. | n. | nw. | | .06 | |
| 2. ● | 30.193 | 30.162 | 29.74 | 30.17 | 30.20 | 72 | 36 | 55 | 51 | 48½ | ne. | nw. | nw. | | | .02 |
| 3. | 30.217 | 30.170 | 29.76 | 30.16 | 30.10 | 73 | 37 | 58 | 54½ | 50½ | ne. | n. | sw. | .01 | | .08 |
| 4. | 30.100 | 30.022 | 29.62 | 29.92 | 29.95 | 78 | 48 | 63 | 52½ | 50½ | nw. | wnw. | nw. | | | .03 |
| 5. | 30.014 | 29.943 | 29.54 | 29.87 | 29.71 | 76 | 45 | 63 | 56 | 50½ | w. | wnw. | wsw. | .02 | | .07 |
| 6. | 30.002 | 29.342 | 29.48 | 29.59 | 29.56 | 77 | 49 | 62½ | 52½ | 48½ | w. | w. | wnw. | .02 | | .27 |
| 7. | 29.769 | 29.308 | 29.23 | 29.45 | 29.48 | 62 | 44 | 56 | 47 | 43½ | sw. | wsw. | wnw. | .25 | .06 | .05 |
| 8. | 29.682 | 28.83 | 28.83 | 29.48 | 29.56 | 48 | 39 | 57 | 49½ | 46 | nw. | nw. | nw. | .48 | .49 | .02 |
| 9. | 29.851 | 29.745 | 29.32 | 29.57 | 29.65 | 64 | 37 | 55 | 50 | 47 | nw. | nw. | wnw. | .01 | .46 | |
| ☽ 10. | 29.926 | 29.910 | 29.50 | 29.72 | 29.75 | 74 | 50 | 65 | 55 | 51½ | nw. | nw. | se. | | .08 | |
| 11. | 29.906 | 29.848 | 29.43 | 29.62 | 29.62 | 73 | 56 | 59 | 57 | 51½ | sw. | wsw. | w. | .02 | | .38 |
| 12. | 29.898 | 29.790 | 29.38 | 29.64 | 29.64 | 70 | 42 | 63 | 54½ | 51 | sw. | w. | calm | .01 | | |
| 13. | 29.993 | 29.981 | 29.47 | 29.71 | 29.80 | 73 | 52 | 70.5 | 54½ | 52 | sw. | nw. | nw. | | | |
| 14. | 29.990 | 29.920 | 29.43 | 29.83 | 29.66 | 77 | 52 | 69 | 58 | 52½ | s. | s. | se. | | .16 | |
| 15. | 29.992 | 29.834 | 29.20 | 29.51 | 29.53 | 68 | 43 | 58 | 61½ | 59½ | sw. | ssw. | se. | .11 | | .22 |
| 16. | 30.104 | 29.737 | 29.56 | 29.64 | 29.77 | 71 | 40 | 61.5 | 54 | 49½ | w. | sw. | nw. | .08 | | .04 |
| ○ 17. | 30.030 | 30.080 | 29.48 | 29.85 | 29.85 | 77 | 49 | 66.5 | 54 | 49 | sw. | w. | nw. | .01 | | .19 |
| 18. | 30.005 | 30.009 | 29.44 | 29.63 | 29.57 | 76 | 61 | 67 | 51 | 52½ | w. | w. | wsw. | | | |
| 19. | 29.997 | 29.979 | 29.48 | 29.71 | 29.85 | 71 | 57 | 69 | 53 | 52½ | nw. | w. | wnw. | | .19 | .34 |
| 20. | 29.991 | 29.941 | 29.39 | 29.89 | 29.93 | 75 | 60 | 67.5 | 53 | 50 | nw. | w. | wnw. | .04 | | .02 |
| 21. | 29.939 | 29.949 | 29.23 | 29.87 | 29.81 | 83 | 47 | 75 | 57 | 50 | se. | nw. | ese. | .07 | .02 | .01 |
| 22. | 29.730 | 29.833 | 29.03 | 29.72 | 29.63 | 86 | 57 | 63 | 65½ | 57 | sw. | sw. | sse. | | | |
| 23. | 29.688 | 29.604 | 29.30 | 29.59 | 29.50 | 77 | 54 | 64 | 58½ | 57 | sw. | sw. | se. | | | |
| ☾ 24. | 29.953 | 29.607 | 29.46 | 29.44 | 29.53 | 76 | 40 | 67 | 60½ | 55 | sw. | w. | calm | | | .02 |
| 25. | 30.060 | 29.801 | 29.57 | 29.65 | 29.67 | 76 | 41 | 65 | 60½ | 53 | sw. | sw. | ssc. | | | .19 |
| 26. | 30.103 | 30.010 | 29.46 | 29.63 | 29.71 | 77 | 50 | 66 | 60 | 56 | sw. | sw. | ssw. | .02 | | |
| 27. | 30.064 | 30.022 | 29.65 | 29.74 | 29.82 | 79 | 50 | 68 | 57 | 54 | sw. | sw. | wsw. | .22 | | .10 |
| 28. | 30.194 | 29.980 | 29.64 | 29.87 | 29.82 | 80 | 51 | 70 | 57½ | 53 | sw. | sw. | wnw. | .08 | | .04 |
| 29. | 30.244 | 30.135 | 29.65 | 29.97 | 30.00 | 87 | 50 | 68 | 57½ | 56 | w. | s. | s. | | .21 | .73 |
| 30. | 30.227 | 30.213 | 29.65 | 29.87 | 29.75 | | | | 56½ | 54 | s. | sw. | w. | | .07 | .17 |
| ● 31. | | 30.161 | 29.67 | 30.01 | 30.04 | 90 | 52 | | 58 | 57 | | sw. | sw. | | | .33 |
| Mean. | 29.998 | 29.906 | 29.45 | 29.758 | 29.770 | 74.51 | 47.90 | 63.6 | 55.35 | 54.20 | | | | 1.43 | 1.80 | 3.36 |

XXXIII. *On the Application of the Mechanical Theory of Heat to the Steam-engine.* By R. CLAUSIUS\*.

1. $\mathbf{A}$S our present modified views respecting the nature and deportment of heat, which constitute the mechanical theory of heat, had their origin in the well-known fact that heat may be employed for producing mechanical work, we may naturally anticipate that the theory so originated will in its turn help to place this application of heat in a clearer light. At all events the more general views thus obtained must enable us to pronounce safely upon the efficiency of the several machines for thus applying heat, as to whether they already perfectly fulfil their purpose, or whether and to what extent they are capable of being perfected.

Besides these reasons, which apply to all thermo-dynamic machines, there are others, applicable more particularly to the most important of them—the steam-engine, which appear to render a new investigation of the latter, conducted according to the principles of the mechanical theory of heat, desirable. It is precisely with respect to vapour at a maximum density that this new theory has led us to laws which differ essentially from those formerly accepted as true, or at least introduced into former calculations.

2. I may here be allowed to refer to a fact proved by Rankine and myself, that when a quantity of vapour at its maximum density, enclosed by a surface impenetrable to heat, expands and thereby displaces a moveable part of the enclosing surface, *e. g.* a piston, with its full force of expansion, a part of the vapour

must undergo condensation; whereas in most works on the steam-engine, amongst others in the excellent work of De Pambour*, Watt's theorem, that under these circumstances the vapour remains precisely at its maximum density, is assumed as a fundamental one.

Further, in the absence of more accurate knowledge, it was formerly assumed, in determining the volumes of the unit of weight of saturated vapour at different temperatures, that vapour even at its maximum density still obeys Mariotte's and Gay-Lussac's laws. In opposition to this, I have already shown in my first memoir† on this subject, that the volumes in question can be calculated from the principles of the mechanical theory of heat under the assumption, *that a permanent gas when it expands at a constant temperature only absorbs so much heat as is consumed in the external work thereby performed,* and that these calculations lead to values which, at least at high temperatures, differ considerably from Mariotte's and Gay-Lussac's laws.

Even the physicists who had occupied themselves more especially with the mechanical theory of heat, did not at that time coincide with this view of the deportment of vapour. William Thomson in particular opposed it. In a memoir‡ presented to the Royal Society of Edinburgh, and published a year later, in March 1851, he only regarded this result as a proof of the improbability of the above assumption which I had employed.

Since then, however, he and J. P. Joule have together undertaken to test experimentally the accuracy of this assumption §. By a series of well-contrived experiments, executed on a large scale, they have in fact shown, that, with respect to the *permanent* gases, atmospheric air and hydrogen, the assumption is so nearly true, that in most calculations the deviations from exactitude may be disregarded. With carbonic acid, the non-permanent gas they investigated, the deviations were greater. This is in perfect accordance with the remark I made on first making the assumption, which was that the latter would probably be found to be accurate in the same measure as Mariotte's and Gay-Lussac's laws were applicable to the gas. In consequence of these experiments, Thomson now calculates the volumes of saturated vapours in the same manner as myself. There is reason to believe, therefore, that the accuracy of this method of calculation will be gradually more and more recognized by other physicists.

---

* *Théorie des Machines à Vapeur*, par le Comte F. M. G. de Pambour. Paris, 1844.
    † Poggendorff's *Annalen*, vol. lxxix. p. 368; Phil. Mag. July 1851.
    ‡ Transactions of the Royal Society of Edinburgh, vol. xx. part 2, p. 261.
    § Phil. Trans. vol. cxliii. part 3, p. 357; and vol. cxliv. part 2, p. 321.

3. These two examples will suffice to show, that the principles upon which our former theory of steam-engines was founded have suffered such essential modifications through the mechanical theory of heat, as to render a new investigation of the subject necessary.

In the present memoir I have attempted to develope the principles of the calculation of the work of the steam-engine in accordance with the mechanical theory of heat. I have, however, limited myself to the steam-engines now in use, without at present entering into a consideration of the more recent and certainly very interesting attempts to employ vapour in a super-heated state.

In recording the results of my investigation, I shall only assume my last memoir, "On a modified Form of the Second Fundamental Theorem in the Mechanical Theory of Heat*," as known. This will of course necessitate the deduction, in a somewhat different manner, of results which are no longer new, but have already been found by myself or others; I believe, however, that this repetition, by leading to greater unity and facility of comprehension, will not be found superfluous. At the proper places I shall, to the best of my ability, cite the works wherein these results first appeared.

4. The expression "*a machine is driven by heat,*" is not of course strictly accurate. By it we must understand, that, in consequence of the changes produced by heat upon some kind of matter in the machine, the parts of the latter are set in motion. We shall refer to this matter as that *which manifests the action of heat.*

If a continuously-acting machine is in uniform action, all accompanying changes occur periodically, so that the condition which at a given time prevails in the machine and all its parts returns at equal intervals. Hence the matter which manifests the action of the applied heat must at such regularly-recurring periods be present in the machine in equal quantity, and in the same state. This condition can be fulfilled in two different ways.

First. One and the same quantity of matter may always remain in the machine, when the modifications which this matter suffers during the action of the machine will be such, that at the end of each period it will regain its original condition and recommence the same cycle of changes.

Secondly. The machine may always expel the matter which served to produce the effect during a period, and in its place receive from an external source just as much matter of the same kind.

5. The last method is the one usually employed in most

* Poggendorff's *Annalen,* vol. xciii. p. 481; and Phil. Mag. August 1856.

machines. This is the case, for instance, in machines with heated air as at present constructed; for after every stroke, the air which moved the piston in the driving cylinder is expelled into the atmosphere, and in its place an equal quantity of air from the same source is received into the feeding cylinder. Similarly in steam-engines without condensers, steam is driven from the cylinder into the atmosphere, and in its place fresh water is pumped from a reservoir into the boiler.

Further, a similar method is at least partially adopted even in steam-engines provided with a condenser as usually constructed. In them the water condensed from the steam is only partially pumped back into the boiler, for being mixed with the cooling water, a part of the latter also reaches the boiler. The remaining part of the condensed water, together with the remaining part of the cooling water, has to be got rid of.

The first method has lately been employed in steam-engines propelled by two vapours, *e. g.* those of water and æther. In these machines the steam is condensed solely by contact with metallic tubes filled with liquid æther, and the water thus produced is then completely pumped back into the boiler. In the same manner the vapour of the æther in the metal tubes, which are merely surrounded by cold water, is condensed and subsequently pumped back into the first space intended for the vaporization of the æther. In order to maintain a uniform action, therefore, only so much fresh water and æther is necessary as will replace the leakage consequent upon imperfect construction.

6. In a machine of this kind, where the same matter is continually re-employed, the several changes which this matter suffers during a period must, as above stated, form a closed cycle, or, according to the nomenclature in my former memoir, a *circular process*.

On the contrary, machines in which a periodical reception and expulsion of matter occurs are not necessarily subject to this condition, though they may also fulfil it by expelling the matter in the same condition in which it was received. This is the case in steam-engines with condensers, where the water is ultimately expelled from the condenser in the liquid state, and at the same temperature as it had when removed from the condenser to the boiler*.

In other machines, the condition, when expelled, is different from what it was when received. For example, heated-air machines, even when provided with regenerators, expel the air

* The cooling water, which enters the condenser at a low and leaves it at a high temperature, is not here taken into consideration, inasmuch as it does not form a part of the matter manifesting the effect of the applied heat, but merely constitutes a negative source of heat.

at a higher temperature than it formerly had ; and steam-engines without condensers receive water in the liquid, and expel it in the gaseous form. Strictly, therefore, the complete circular process is not fulfilled in these cases ; nevertheless we may always conceive a second machine appended to the given one which shall receive the matter from the first, reduce it in some manner to its original condition, and then expel it. Both machines may then be regarded as constituting one and the same machine, which will fulfil the above condition. In many cases this addition may be made without introducing greater complexity into the investigation. For example, a steam-engine with a condenser at a temperature of 100° C. may be substituted for a machine without a condenser, provided we assume the latter to be fed with water at 100° C.

Hence, if we assume that machines which do not fulfil the above condition are theoretically completed in the above manner, we may apply the theorems concerning the circular process to all thermo-dynamic machines, and thereby arrive at conclusions which are quite independent of the nature of the processes executed by the several machines.

7. In my former memoir I have represented the two fundamental theorems which hold good in every circular process by the following equations :—

$$Q = A \cdot W \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(I)}$$

$$\int \frac{dQ}{T} = -N, \quad . \quad . \quad . \quad . \quad . \quad \text{(II)}$$

wherein the letters have the same signification as before, viz.—

A is the equivalent of heat for the unit of work.

W represents the external work performed during the circular process.

Q signifies the heat imparted to the changeable body during a circular process, and $dQ$ an element of the same, whereby any heat withdrawn from the body is to be considered as an imparted negative quantity of heat. The integral in the second equation is extended over the whole quantity Q.

T is a function of the temperature which the changing matter has at the moment when it receives the element of heat $dQ$ ; or should the temperature of different parts of the body be different, a function of the temperature of the part which receives $dQ$. With respect to the form of the function T, I have shown in my former memoir that it is probably the temperature itself reckoned from a point which may be determined from the reciprocal value of the coefficient of expansion of an ideal gas, and which must be in the neighbourhood of −273° C. ; so that if $t$ represents

the temperature above the freezing-point,

$$\mathrm{T}=273+t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In the present memoir T will always have this signification, and for brevity will be called the *absolute temperature.* It may be here remarked, however, that the conclusions do not essentially depend upon this signification, but remain true even when T is considered as an undetermined function of the temperature.

. Lastly, N denotes the equivalence-value of all the uncompensated transformations* involved in a circular process.

8. If the process is such that it can be reversed in the same manner, then N=0. If, however, one or more changes of condition occur in a circular process which are not reversible, then uncompensated transformations necessarily arise, and the magnitude N has, consequently, a determinable and necessarily positive value.

Amongst the operations to which the last remark is applicable, is one which in the following will be often mentioned. When a quantity of gas or vapour expands, and thereby overcomes a pressure equivalent to its total expansive force, it may be again compressed into its former volume by employing the same power, when all the phænomena which accompanied the expansion will take place in an inverse manner. This is not the case, however,

---

* One species of uncompensated transformations requires further remark. The sources from which the changing matter derives heat must have higher temperatures than itself; and, on the other hand, those from which it derives negative quantities of heat, or which deprive it of heat, must have lower temperatures than itself. But whenever heat is interchanged between the changing body and any source whatever, heat passes immediately from the body at a higher to the one at a lower temperature, and thus an uncompensated transformation occurs which is greater the greater the difference between the temperatures. In determining such uncompensated transformations, not only must the changes in the condition of the variable matter be taken into consideration, but also the temperatures of the sources of heat which are employed; and these uncompensated transformations will be included in N or not, according to the signification which is attached to the temperature occurring in equation (II). If thereby the temperature of the *source of heat* belonging to $d\mathrm{Q}$ is understood, the above changes will be included in N. If, however, agreeably to the above definition, and to our intention throughout this memoir, the temperature of the changing matter is understood, then the above transformations are excluded from N. One more remark must be added concerning the minus sign prefixed to N, which did not appear in the same equation in my former memoir. This difference arises from the different application of the terms negative and positive with respect to quantities of heat. Before, a quantity of heat received by the changeable body was considered as negative because it was lost by the source of heat; now, however, it is considered as positive. Hereby every element of heat embraced by the integral, and consequently the integral itself, changes its sign; and hence, to preserve the correctness of the equation, the sign on the other side must be changed.

when the gas or vapour does not, during its expansion, encounter all the resistance it is capable of overcoming; when, for instance, it issues from a vessel in which the pressure is greater than in the one into which it enters. In this case a compression, under circumstances similar to those accompanying expansion, is impossible.

By equation (II) we can determine the sum of all the uncompensated transformations in a circular process. As, however, a circular process may consist of several changes of condition in the given matter, of which some have occurred in a reversible, and others in an irreversible manner, it is often interesting to know how much of the whole sum of uncompensated transformations has resulted from each of the latter. For this purpose let us conceive the matter, after the modification which has to be examined in this manner, reduced to its original condition by any reversible operation. We shall thereby obtain a small circular process, to which the equation (II) will be just as applicable as to the whole. Consequently, if we know the quantities of heat which the matter has received during the process, and the temperatures which correspond thereto, the negative integral $-\int \frac{dQ}{T}$ will give the uncompensated transformation involved therein. But as the uncompensated transformation involved in the given change of condition could not have been increased by the above reduction, which was executed in a reversible manner, it will be fully represented by the above expression.

Having thus investigated all the parts of the whole circular process which are not reversible, and found the values $N_1$, $N_2$, &c., which must all be positive, their sum will give the magnitude $N$ corresponding to the whole circular process, without its being necessary to take into consideration those parts which are known to be reversible.

9. If we now apply the equations (I) and (II) to the circular process which occurs during a period in a thermo-dynamic machine, it will be at once evident, that, the whole quantity of heat communicated during this period to the matter in the machine being given, the corresponding amount of work can be immediately determined from the first equation without its being necessary to know the nature of the operations constituting the circular process.

In an equally general manner the work may be determined from other data by a combination of both equations.

We will assume that the quantities of heat successively imparted to the changing material, as well as the temperatures at the times of reception, are given, and that only one temperature, $T_0$, remains at which a certain as yet unknown quantity of heat

was imparted or, if negative, abstracted. The sums of all the known and unknown quantities of heat shall be represented by $Q_1$ and $Q_0$ respectively.

Divide the integral in equation (II) into two parts, of which the one shall extend over the known quantity of heat $Q_1$, and the other over the unknown quantity $Q_0$. In the latter part, T having a constant value $T_0$, the integration may be immediately effected, and as result we have

$$\frac{Q_0}{T_0}.$$

The equation (II) thus becomes

$$\int_0^{Q_1} \frac{dQ}{T} + \frac{Q_0}{T_0} = -N,$$

whence results

$$Q_0 = -T_0 . \int_0^{Q_1} \frac{dQ}{T} - T_0 . N.$$

Further, seeing that in our case

$$Q = Q_1 + Q_0,$$

we have from equation (I),

$$W = \frac{1}{A} (Q_1 + Q_0).$$

Substituting the above-found value of $Q_0$ in this equation, it becomes

$$W = \frac{1}{A} \left( Q_1 - T_0 \int_0^{Q_1} \frac{dQ}{T} - T_0 . N \right). \quad . \quad . \quad (2)$$

If, as a special case, the whole circular process is reversible, then

$$N = 0,$$

and the above equation becomes

$$W = \frac{1}{A} \left( Q_1 - T_0 . \int_0^{Q_1} \frac{dQ}{T} \right). \quad . \quad . \quad (3)$$

This expression differs from the preceding one only in the absence of the term $-\dfrac{T_0}{A} N$. Now as N can only be positive, this term must necessarily be negative; and thus we see, that, under the above conditions with respect to the communication of heat, the greatest possible amount of work is obtained when the whole process is reversible; and that every circumstance which renders one of the operations in the circular process not reversible, dimi-

nishes the amount of work,—a conclusion which results easily from a direct consideration of the subject.

The equation (2) leads to the value of the amount of work in a manner opposite to that usually followed. The amounts of work done in the several operations are not separately determined and then added together, but, instead of this, the maximum of work is first found, and the losses occasioned by the several imperfections of the process are subsequently deducted from it.

If, with respect to the communication of heat, we introduce a still more limited condition, and assume that the whole quantity of heat $Q_1$ is also imparted to the body at a constant temperature $T_1$, then the integration which embraces this quantity of heat may also be executed, and gives

$$\frac{Q_1}{T_1},$$

whereby the equation (3) for the maximum of work assumes the form

$$W = \frac{Q_1}{A} \cdot \frac{T_1 - T_0}{T_1}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

In this special form the equation has already been deduced by William Thomson and Rankine from a combination of Carnot's theorem, as modified by me, and the theorem of the equivalence of heat and work*.

10. Before we proceed, from these considerations which apply to all thermo-dynamic engines, to treat of the steam-engine, we must first premise something concerning the deportment of vapours at a maximum density.

In a memoir of mine, published as early as 1850, " On the Moving Force of Heat," &c., I have already established the equations which show the application of the two fundamental theorems of the mechanical theory of heat to vapours at a maximum density, and I have there employed these equations in deducing several consequences. But as in my last memoir, " On a modified Form of the Second Fundamental Theorem of the Mechanical Theory of Heat," I proposed a somewhat different mode of treating the whole subject, it appears preferable to me to assume the last memoir only as known. I shall therefore deduce those equations once more, in a different manner, by means of the results established in my last memoir.

It was there assumed, in order to apply the general equations which were first established to a somewhat more special case, that the only foreign force, acting upon the changing material, which required consideration in determining the external work,

* Phil. Mag. July 1851.

was an external pressure equally intense at all points of the surface, and directed everywhere at right angles to the same; and further, that this pressure always changed so slowly, and consequently at each moment differed so little from the opposite expansive force of the body, that in calculation the two might be considered equal. Let then $p$ be the pressure, $v$ the volume, and T the absolute temperature of the body. We introduce the last instead of $t$, the temperature counted from the freezing-point, because thereby the formulæ assume a simpler form. The equations already established in this case are,

$$\frac{d}{dT}\left(\frac{dQ}{dv}\right) - \frac{d}{dv}\left(\frac{dQ}{dT}\right) = A \cdot \frac{dp}{dT}, \quad \cdot \quad \cdot \quad \cdot \quad \text{(III)}$$

$$\frac{dQ}{dv} = A \cdot T \frac{dp}{dT}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(IV)}$$

These equations shall next be applied to the still more special case of vapours at their maximum density.

11. Let M be the mass of the matter whose vapour is to be considered, and which is placed in a perfectly closed expansible vessel. Let the part $m$ be in a vaporous, and the rest, $M - m$, in a liquid state. This mixed mass shall be the changing body to which the foregoing equations are to be referred.

The condition of the mass, as far as the same here enters into consideration, is perfectly determined as soon as its temperature T and its volume $v$, *i. e.* the volume of the vessel, are given. For, according to hypothesis, the vapour is always in contact with the liquid, and therefore remains at its maximum density; so that its condition, as well as that of the liquid, depends only upon the temperature T. It only remains to be seen, therefore, whether the magnitude of each of the parts in different conditions is perfectly determined, from the condition that both parts together exactly fill the space enclosed by the vessel. Let $s$ represent the volume of the unit of weight of vapour at its maximum density where the temperature is T, and $\sigma$ that of the unit of weight of liquid, then

$$v = m \cdot s + (M - m)\sigma$$
$$= m(s - \sigma) + M\sigma.$$

The magnitude $s$ never occurs hereafter except in the combination $s - \sigma$, so that we will introduce another letter for this difference, and make

$$u = s - \sigma, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(5)}$$

in consequence of which the foregoing equation becomes

$$v = mu + M\sigma, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(6)}$$

and we have

$$m = \frac{v - M\sigma}{u}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

By this equation $m$ is expressed as a function of T and $v$, because $u$ and $\sigma$ are functions of T.

12. In order to be able to apply equations (III) and (IV) to our case, we must next determine the magnitudes $\frac{d Q}{d v}$ and $\frac{d Q}{d T}$.

If the volume of the vessel increases by $dv$, then the quantity of heat which must be imparted to the mass in order to maintain a constant temperature will be generally expressed by

$$\frac{d Q}{d v} dv.$$

But this quantity of heat is expended solely in the vaporization which takes place during the expansion; so that if $r$ represents the heat required to vaporize the unit of mass, the above quantity of heat may also be represented by

$$r \frac{d m}{d v} dv,$$

and we have

$$\frac{d Q}{d v} = r \frac{d m}{d v}.$$

But according to (7),

$$\frac{d m}{d v} = \frac{1}{u};$$

hence

$$\frac{d Q}{d v} = \frac{r}{u}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Let us next assume, that whilst the volume of the vessel remains constant, the temperature of the mass increases by $dT$; then the general expression for the requisite quantity of heat will be

$$\frac{d Q}{d T} dT.$$

This quantity of heat consists of three parts:—

(1) The liquid part $M - m$ of the whole mass suffers an increment of temperature $dT$, for which, $c$ being the specific heat of the liquid, the quantity of heat

$$(M - m)c \, dT$$

is necessary.

(2) The vaporous part $m$ will also undergo an increment of temperature $dT$, but it will be thereby compressed so as still to

remain at its maximum density for the increased temperature $T + dT$. For an increment of temperature $dT$, we will represent by $h \cdot dT$ the quantity of heat which must be imparted to the unit of mass of vapour during its contraction, in order that at every density it may have precisely that temperature for which this density is a maximum. The value and even the sign of the magnitude $h$ is at present unknown. The quantity of heat necessary in our case will therefore be

$$mh\,dT.$$

(3) During the elevation of temperature, a small quantity of liquid, represented generally by

$$\frac{dm}{dT}\,dT,$$

becomes vaporized, for which the quantity of heat

$$r\frac{dm}{dT}dT$$

is necessary. Herein, according to equation (7),

$$\frac{dm}{dT} = -\frac{v - M\sigma}{u^2} \cdot \frac{du}{dT} - \frac{M}{u} \cdot \frac{d\sigma}{dT}$$

$$= -\frac{m}{u} \cdot \frac{du}{dT} - \frac{M}{u} \cdot \frac{d\sigma}{dT};$$

so that by substitution the last expression becomes

$$-r\left(\frac{m}{u} \cdot \frac{du}{dT} + \frac{M}{u} \cdot \frac{d\sigma}{dT}\right)dT.$$

Equating the sum of these three quantities of heat and the former expression $\dfrac{dQ}{dT}\,dT$, we obtain the equation

$$\frac{dQ}{dT} = M\left(c - \frac{r}{u} \cdot \frac{d\sigma}{dT}\right) + m\left(h - c - \frac{r}{u} \cdot \frac{du}{dT}\right). \qquad (9)$$

13. As indicated by equation (III), the above expression for $\dfrac{dQ}{dv}$ must be differentiated according to $T$, and the expression for $\dfrac{dQ}{dT}$ according to $v$. The magnitude $M$ is constant, the magnitudes $u$, $\sigma$, $r$, $c$, and $h$ are all functions of $T$ alone, and only the magnitude $m$ is a function of $T$ and $v$, so that

$$\left.\begin{aligned}
\frac{d}{dT}\left(\frac{dQ}{dv}\right) &= \frac{1}{u} \cdot \frac{dr}{dT} - \frac{r}{u^2} \cdot \frac{du}{dT} \\
\frac{d}{dv}\left(\frac{dQ}{dT}\right) &= \left(h - c - \frac{r}{u} \cdot \frac{du}{dT}\right)\frac{dm}{dv};
\end{aligned}\right\} \qquad (10)$$

or, substituting for $\frac{dm}{dv}$ its value $\frac{1}{u}$,

$$\frac{d}{dv}\left(\frac{dQ}{dT}\right) = \frac{h-c}{u} - \frac{r}{u^2} \cdot \frac{du}{dT}. \quad \ldots \quad (11)$$

By substituting the expressions given in (10), (11), and (8) in (III) and (IV), we obtain the required equations, which represent the two principal theorems of the mechanical theory of heat as applied to vapours at their maximum density. These are

$$\frac{dr}{dT} + c - h = A \cdot u \frac{dp}{dT}, \quad \ldots \quad (V)$$

$$r = A \cdot Tu \frac{dp}{dT}; \quad \ldots \quad (VI)$$

and from a combination of both we have

$$\frac{dr}{dT} + c - h = \frac{r}{T}. \quad \ldots \quad (12)$$

14. By means of these equations we will now treat a case, which in the following will so frequently occur, as to render it desirable at once to establish the results which have reference thereto.

Let us suppose that the vessel before considered, containing the liquid and vaporous parts of the mass, changes its volume *without heat being imparted to, or withdrawn from, the mass.* Then, simultaneously with the volume, the temperature and magnitude of the vaporous part of the mass will change; and besides this — seeing that during the change of volume the pressure of the enclosed vapour is active, which pressure during expansion overcomes, and during contraction is overcome by an external force — a positive or negative amount of external work will be done by the heat which produces the pressure.

*Under these circumstances, the magnitude of the vaporous part* m, *the volume* v, *and the work* W *shall be determined as functions of the temperature* T.

15. It has already been shown, that, in order that the volume and temperature may suffer any infinitely small increments $dv$ and $dT$, a quantity of heat expressed by the sum

$$r \frac{dm}{dv} dv + \left[ (M - m)c + mh + r \frac{dm}{dT} \right] dT$$

must be imparted to the mass. In consequence of the present condition, according to which heat is neither imparted to, nor abstracted from the mass, this sum must be set equal to zero.

Accordingly, writing $dm$ in place of

$$\frac{dm}{dv}\,dv + \frac{dm}{d\mathrm{T}}\,d\mathrm{T},$$

we obtain the equation

$$rdm + m(h-c)d\mathrm{T} + \mathrm{M}cd\mathrm{T} = 0. \quad . \quad . \quad . \quad (13)$$

But by (12),

$$h-c = \frac{dr}{d\mathrm{T}} - \frac{r}{\mathrm{T}};$$

so that writing $dr$ in the place of $\frac{dr}{d\mathrm{T}}\,d\mathrm{T}$, $r$ being a function of T alone, we have

$$rdm + mdr - \frac{mr}{\mathrm{T}}\,d\mathrm{T} + \mathrm{M}cd\mathrm{T} = 0,$$

or

$$d(mr) - \frac{mr}{\mathrm{T}}\,d\mathrm{T} + \mathrm{M}cd\mathrm{T} = 0. \quad . \quad . \quad . \quad . \quad (14)$$

This equation, divided by T, becomes

$$\frac{d(mr)}{\mathrm{T}} - \frac{mr}{\mathrm{T}^2}d\mathrm{T} + \mathrm{M}c\frac{d\mathrm{T}}{\mathrm{T}} = 0,$$

or

$$d\left(\frac{mr}{\mathrm{T}}\right) + \mathrm{M}c\frac{d\mathrm{T}}{\mathrm{T}} = 0. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Inasmuch as the specific heat of a liquid changes only very slowly with its temperature, we will in future always consider the magnitude $c$ as constant. In this case the above equation can be immediately integrated, and gives

$$\frac{mr}{\mathrm{T}} + \mathrm{M}c \log \mathrm{T} = \text{const.};$$

or if $\mathrm{T}_1$, $r_1$, and $m_1$ be the initial values of T, $r$, and $m$,

$$\frac{mr}{\mathrm{T}} = \frac{m_1 r_1}{\mathrm{T}_1} - \mathrm{M}c \log \frac{\mathrm{T}}{\mathrm{T}_1}. \quad . \quad . \quad . \quad (\mathrm{VII})$$

If $r$ may be considered as a known function of the temperature, as through Regnault's experiments it may be in the case of steam, then by means of this equation $m$ is also expressed as a function of the temperature.

In order to give some idea of the deportment of this function, I have, for one particular case, collected together a few calculated values in the following Table. For instance, it is assumed that at the commencement the vessel contains no water in a liquid state, but is exactly filled with vapour at a maximum density, so that $m_1 = \mathrm{M}$; and that an expansion of the vessel now takes

place. If the vessel has to be compressed, then the assumption that at the commencement it contained no liquid could not be made, because in such a case the vapour would not remain at a maximum density, but would become over-heated by the heat generated through compression. During expansion, however, not only does the vapour remain at a maximum density, but a part of it is actually condensed; and it is the diminution of $m$ consequent thereon which is exhibited in the Table. The initial temperature is supposed to be 150° C., and the values of $\frac{m}{M}$ are given which correspond to the periods when, by expansion, the temperature is reduced to 125°, 100°, &c. . . . . As before, in order to distinguish it from the absolute temperature T, the temperature counted from the freezing-point is represented by $t$.

| $t$. | 150° | 125° | 100° | 75° | 50° | 25° |
|------|------|------|------|-----|-----|-----|
| $\frac{m}{M}$ | 1 | 0·956 | 0·911 | 0·866 | 0·821 | 0·776 |

16. In order to express the relation which exists between the volume $v$ and the temperature, we must employ the equation (6), *i. e.*

$$v = mu + M\sigma.$$

The magnitude $\sigma$ herein involved, which represents the volume of a unit of weight of liquid, changes very little with the temperature; and these small changes may be the more safely neglected, because the whole value of $\sigma$ is very small in comparison to $u$; we shall consequently consider $\sigma$, as well as the product $M\sigma$, as constant. The product $mu$ therefore alone remains to be determined. For this purpose we have only to substitute the value of $r$, as given in equation (VI), in equation (VII), and we obtain

$$mu\frac{dp}{dT} = m_1 u_1 \left(\frac{dp}{dT}\right)_1 - \frac{Mc}{A}\log\frac{T}{T_1}. \quad \text{(VIII)}$$

The differential coefficient $\frac{dp}{dT}$ which here appears, is to be considered as known, $p$ itself being given as a function of the temperature. Hence the product $mu$ is determined by this equation, and by the addition of $M\sigma$ the required value of $v$ will also result from it.

The same assumptions being made as before, the following Table shows a series of values of the fraction $\frac{v}{v_1}$ calculated from this equation. For the sake of comparison are also appended

the values of $\frac{v}{v_1}$, which would be obtained if the two assumptions formerly made in the theory of the steam-engine were correct; that is to say, (1) that the vapour during expansion remains without partial condensation at a maximum density; (2) that it follows Mariotte's and Gay-Lussac's laws. According to these hypotheses, we should have

$$\frac{v}{v_1} = \frac{p_1}{p} \cdot \frac{T}{T_1}.$$

| $t$. | 150° | 125° | 100° | 75° | 50° | 25° |
|---|---|---|---|---|---|---|
| $\frac{v}{v_1}$ | 1 | 1·88 | 3·90 | 9·23 | 25·7 | 88·7 |
| $\frac{p_1}{p} \cdot \frac{T}{T_1}$ | 1 | 1·93 | 4·16 | 10·21 | 29·7 | 107·1 |

17. We have still to determine the work done during the change of volume. In order to do so, we have the general equation

$$W = \int_{v_1}^{v} p\,dv. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

But, considering $\sigma$ constant, we have from equation (6),

$$dv = d(mu);$$

therefore

$$pdv = pd(mu),$$

for which we may also write

$$pdv = d(mup) - mu\frac{dp}{dT}\,dT. \quad . \quad . \quad . \quad (17)$$

In the place of $mu\frac{dp}{dT}$ we might here substitute the expression given in (VIII), and then integrate; but the result is at once obtained in a rather more convenient form by the following substitution. According to (VI),

$$mu\frac{dp}{dT}\,dt = \frac{1}{A} \cdot \frac{mr}{T}dT;$$

and through the application of equation (14), this becomes

$$mu\frac{dp}{dT}\,dT = \frac{1}{A}\left[d(mr) + McdT\right].$$

By means of this (17) becomes

$$pdv = d(mup) - \frac{1}{A}\left[d(mr) + McdT\right];$$

and integrating this equation, we have

$$\mathrm{W} = mup - m_1 u_1 p_1 + \frac{1}{\mathrm{A}}[m_1 r_1 - mr + \mathrm{M}c(\mathrm{T}_1 - \mathrm{T})], \quad \text{(IX)}$$

whence, the magnitudes $mr$ and $mu$ being already known from former equations, W may be calculated.

I have also made this calculation for the above special case, and given the values of $\frac{\mathrm{W}}{\mathrm{M}}$, *i. e.* of the work done during expansion by the unit of mass, in the following Table. A kilogramme is chosen as unit of mass, and a kilogramme-metre as unit of work. For $\frac{1}{\mathrm{A}}$, the value 423·55, as found by Joule, is employed*.

For the sake of comparison with the numbers in the Table, it may be well to state that when 1 kilogramme of water is evaporated at the temperature of 150°, and under the corresponding pressure, the quantity of work done by the vapour during its formation in overcoming the external counter-pressure has the value 18700.

| $t.$ | 150° | 125° | 100° | 75° | 50° | 25° |
|---|---|---|---|---|---|---|
| $\frac{\mathrm{W}}{\mathrm{M}}$ | 0 | 11300 | 23200 | 35900 | 49300 | 63700 |

18. We proceed now to the consideration of the steam-engine itself.

In the adjoining figure, which is intended merely to facilitate our oversight of the whole series of operations involved in the working of a common steam-engine, A represents the boiler whose contents are maintained by the source of heat at a constant temperature $\mathrm{T}_1$. A part of the steam passes from the boiler to the cylinder B and raises the piston a certain height. The cylinder and boiler are next disconnected, and
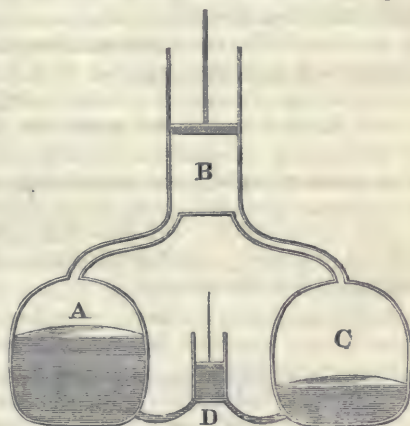


* $\frac{1}{\mathrm{A}}$ is the equivalent of work for the unit of heat; and the above number denotes, therefore, that the quantity of heat which can raise a kilogramme of water from 0° to 1° C., when converted into mechanical work, gives an amount equal to 423·55 kilogramme-metres.

the vapour contained in the former raises the piston still higher by its own expansion. After this the cylinder is put in communication with the space C, which shall represent the condenser. We shall suppose the latter to be kept cold by external cooling, and not by injected water, which, as before remarked, causes no essential difference in the results, and yet simplifies our problem. The constant temperature of the condenser shall be $T_0$. During the connexion of the cylinder with the condenser the piston retraces the whole of its former path, and thus all the vapour which did not immediately pass by itself into the condenser is driven into it, and there becomes condensed. In order to complete the cycle of operations, it is now necessary to convey the liquid produced by condensation back again into the boiler. This is done by means of the small pump D, whose action is so regulated, that at every ascent of the piston just as much liquid is withdrawn from the condenser as entered it by the above condensation; and during the descent of the piston this same quantity of liquid is forced back into the boiler. As soon as this liquid is again raised in the boiler to the temperature $T_1$, everything is once more in its initial condition, and the same series of operations can commence again. Here, therefore, we have a complete circular process.

In ordinary steam-engines the steam enters the cylinder not only at one end, but alternately at both. But the only difference produced thereby is, that during an ascent and descent of the piston, two circular processes take place instead of one; and in this case even the determination of the work for one of the processes is sufficient, because from it the total amount of work done during any time can be deduced*.

19. In making this determination, we shall, as is indeed usual in such cases, consider the cylinder as impenetrable to heat, so that we may neglect the interchange of heat which takes place during a stroke between the walls of the cylinder and the vapour.

The mass in the cylinder can only consist of vapour *at a maximum density*, together with some admixed liquid. For it is evident from the foregoing, that during its expansion in the cylinder, after the latter is cut off from the boiler, the vapour cannot pass into the over-heated condition, but must, on the contrary, be partially condensed, provided no heat reaches it from an external source. In other operations hereafter to be mentioned, where this over-heated state might certainly occur, it will be prevented by the small amount of liquid which the vapour always carries with it into the cylinder, and with which it remains in contact.

The quantity of liquid thus mixed with the vapour is incon-

* The space on one side of the piston is a little diminished by the piston-rod, but an allowance can easily be made for this small difference.

siderable; and as it is for the most part distributed throughout the vapour in small drops, so that it can readily participate in any changes of temperature which the vapour may suffer during expansion, we shall incur no great inaccuracy if, in calculation, we consider the temperature at any moment as the same throughout the whole of the mass in the cylinder.

Further, in order to avoid complicating our formulas too much, we will for the present determine the total amount of work done by the vapour pressure, without taking into consideration how much of this work is useful, and how much is again consumed by the machine itself in overcoming friction, and in working any pumps, which, besides the one in the figure, may be necessary to the efficiency of the machine. This part of the work may be afterwards determined and deducted, as will subsequently be shown.

With respect to the friction of the piston in the cylinder, however, we may remark, the work consumed in overcoming it can scarcely be considered as lost. For heat is generated by this friction, and consequently the interior of the cylinder kept warmer than it would otherwise be, and thus the force of the vapour increased.

Lastly, inasmuch as it is advisable first to study the actions of the most perfect machines before examining the influence of the several imperfections which practically are always unavoidable, we will add to these preliminary considerations two more suppositions, which shall afterwards be again relinquished. First, the canal from the boiler to the cylinder, and that from the cylinder to the condenser, or to the atmosphere, shall be so wide, or the speed of the machine shall be so slow, that the pressure in the part of the cylinder in connexion with the boiler shall be equal to that in the boiler itself, and similarly the pressure on the other side of the piston shall be equal to the pressure in the condenser or to the atmospheric pressure; and secondly, no vicious space shall be present.

20. Under these circumstances, the quantities of work done during a circular process can be written down, without further calculation, by help of the results above attained; and for their sum they give a simple expression.

Let M be the whole mass which passes from the boiler into the cylinder during the ascent of the piston, and of it let $m_1$ be the vaporous, and $M - m_1$ the liquid part. The space occupied by this mass is

$$m_1 u_1 + M\sigma;$$

where $u_1$ is the value of $u$ corresponding to $T_1$. The piston is raised therefore until this space is left free under it; and as this takes place under the action of the pressure $p_1$, corresponding to

S 2

$T_1$, the work performed during this first operation is

$$W_1 = m_1 u_1 p_1 + M\sigma p_1. \quad . \quad . \quad . \quad . \quad (18)$$

The expansion which now follows is continued until the temperature of the mass enclosed in the cylinder sinks from $T_1$ to a second given value $T_2$. The work thus done, which shall be $W_2$, is given immediately by equation (IX), if $T_2$ be taken therein as the final temperature, and for the other magnitudes involved in the equation the corresponding values be substituted, thus

$$W_2 = m_2 u_2 p_2 - m_1 u_1 p_1 + \frac{1}{A}[m_1 r_1 - m_2 r_2 + Mc(T_1 - T_2)]. \quad (19)$$

By the descent of the piston, which now commences, the mass, which at the close of the expansion occupied the volume

$$m_2 u_2 + M\sigma,$$

is driven from the cylinder into the condenser, and has to overcome the constant pressure $p_0$. The negative work hereby done by this pressure is

$$W_3 = - m_2 u_2 p_0 - M\sigma p_0. \quad . \quad . \quad . \quad (20)$$

Whilst the piston of the small pump now ascends, so as to leave the free space $M\sigma$ under it, the pressure $p_0$ in the condenser acts favourably and does the work,

$$W_4 = M\sigma p_0. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Lastly, during the descent of this piston, the pressure $p_1$ in the boiler must be overcome, and therefore it does the negative work,

$$W_5 = - M\sigma p_1. \quad . \quad . \quad . \quad . \quad . \quad (22)$$

By adding these five magnitudes together we obtain the following expression for the work done by the vapour pressure, or, as we may say, by heat, during a circular process:

$$W' = \frac{1}{A}[m_1 r_1 - m_2 r_2 + Mc(T_1 - T_2)] + m_2 u_2 (p_2 - p_0). \quad . \quad (X)$$

With respect to the magnitude $m_2$, which must be eliminated from this equation, it will be observed that, if for $u_2$ we substitute the value

$$\frac{r_2}{A \cdot T_2 \left(\dfrac{dp}{dT}\right)_2},$$

as given in (VI), it only occurs in the combination $m_2 r_2$, and for this product we have from equation (VII) the expression

$$m_2 r_2 = m_1 r_1 \frac{T_2}{T_1} - McT_2 \log \frac{T_2}{T_1}.$$

By employing this expression, therefore, we obtain an equation the right-hand side of which contains only known quantities; for the masses $m_1$ and M, and the temperatures $T_1$, $T_2$, and $T_0$, are assumed to be immediately given, and the magnitudes $r$, $p$, and $\dfrac{dp}{dT}$ are supposed to be known functions of the temperature.

21. If in the equation (X) we set $T_2 = T_1$, we find the amount of work, for the case that the machine works without expansion, to be

$$W' = m_1 u_1 (p_1 - p_0). \quad \ldots \quad \ldots \quad (23)$$

If, on the contrary, we suppose the expansion to be continued until the vapour sinks from the temperature of the boiler to that of the condenser,—which case cannot of course be strictly realized, but rather forms a limit which it is desirable to approach as much as possible,—we have only to set $T_2 = T_0$, when we obtain

$$W' = \frac{1}{A} [m_1 r_1 - m_0 r_0 + Mc(T_1 - T_0)]. \quad \ldots \quad (24)$$

Eliminating $m_0 r_0$ by means of the equation before given, in which we must also set $T_2 = T_0$, we have

$$W' = \frac{1}{A} \left[ m_1 r_1 \frac{T_1 - T_0}{T_1} + Mc \left( T_1 - T_0 + T_0 \log \frac{T_0}{T_1} \right) \right]^{*}. \quad (XI)$$

22. If to the foregoing equation we give the form

$$W' = m_1 r_1 \cdot \frac{T_1 - T_0}{AT_1} + Mc(T_1 - T_0) \cdot \frac{1}{A} \left( 1 + \frac{T_0}{T_1 - T_0} \log \frac{T_0}{T_1} \right), \quad (25)$$

then the two products $Mc(T_1 - T_0)$ and $m_1 r_1$ which appear therein together represent the quantity of heat furnished by the source of heat during a circular process. For the first is the quantity of heat which is necessary to raise the temperature of the liquid mass M, coming from the condenser, from $T_0$ to $T_1$; and the

* The above equations, representing the amount of work under the two simplifying conditions introduced at the close of § 19, were developed by me some time ago, and publicly communicated in my lectures at the Berlin University as early as the summer of 1854. Afterwards, on the appearance, in 1855, of the Philosophical Transactions for the year 1854, I found therein a memoir of Rankine's, "On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermo-dynamic Engines," and was surprised to find that at about the same time Rankine, quite independently, and in a different manner, arrived at equations which almost entirely agreed with mine, not only in their essential contents, but even in their forms; with this exception only, that Rankine did not consider, that, when entering the cylinder, a quantity of liquid is mixed with the vapour. By the earlier publication of this memoir I lost, of course, all claim to priority with respect to this part of my investigations; nevertheless the agreement was so far satisfactory as to furnish me with a guarantee for the accuracy of the method I had employed.

latter is the quantity consumed in vaporizing the part $m_1$ at the temperature $T_1$. As $m_1$ is but little smaller than M, the last quantity of heat is far greater than the first.

In order more conveniently to compare the two factors with which these two quantities of heat are multiplied in equation (25), we will alter the form of the one which multiplies $Mc(T_1 - T_0)$. If, for brevity, we make

$$z = \frac{T_1 - T_0}{T_1}, \quad \ldots \quad \ldots \quad (26)$$

then

$$\frac{T_0}{T_1 - T_0} = \frac{1-z}{z},$$

and

$$\frac{T_0}{T_1} = 1 - z;$$

so that we have

$$
\begin{aligned}
1 + \frac{T_0}{T_1 - T_0} \log \frac{T_0}{T_1} &= 1 + \frac{1-z}{z} \log (1-z) \\
&= 1 - \frac{1-z}{z} \left( \frac{z}{1} + \frac{z^2}{2} + \frac{z^3}{3} + \&c. \right) \\
&= \frac{z}{1.2} + \frac{z^2}{2.3} + \frac{z^3}{3.4} + \&c. \ldots
\end{aligned}
$$

Hence the equation (25) or (XI) becomes

$$W' = m_1 r_1 \cdot \frac{z}{A} + Mc(T_1 - T_0) \cdot \frac{z}{A} \left( \frac{1}{1.2} + \frac{z}{2.3} + \frac{z^2}{3.4} + \&c. \ldots \right) (27)$$

It is easy to see that the value of the infinite series, which distinguishes the factor of the quantity of heat $Mc(T_1 - T_0)$ from that of the quantity of heat $m_1 r_1$, varies from $\frac{1}{2}$ to 1, as $z$ increases from 0 to 1.

23. In the case last considered, where the vapour by expansion cools down to the temperature of the condenser, we can easily obtain the expression for the work done in another manner, without considering the several operations which constitute the circular process.

For in this case every part of the circular process is reversible. We can imagine that the vaporization takes place in the condenser at the temperature $T_0$, and that the mass M, of which $m_0$ is vaporous and $M - m_0$ liquid, enters the cylinder and raises the piston; further, that by the descent of the piston the vapour is first compressed until its temperature is raised to $T_1$, and then that it is forced into the boiler; and lastly, that by means of the small pump the mass M is again conveyed in the liquid form from the boiler to the condenser, and allowed to cool there to

the original temperature $T_0$. The matter here passes through the same conditions as before, but in an opposite order. All communications and abstractions of heat take place in opposite order, but in the same quantity and at the same temperature of the mass; all quantities of work have opposite signs, but the same numerical value.

Hence it follows, that in this case no uncompensated transformation is involved in the circular process, and we must consequently set $N=0$ in equation (2), by which we obtain the following equation,—already given in (3), with the exception that $W'$ is here put in the place of $W$,—

$$W' = \frac{1}{A}\left( Q_1 - T_0 \int_0^{Q_1} \frac{dQ}{T} \right).$$

In our present case, $Q_1$ denotes the quantity of heat imparted to the mass M in the boiler, that is,

$$Q_1 = m_1 r_1 + Mc(T_1 - T_0).$$

In determining the integral $\int_0^{Q_1} \frac{dQ}{T}$, the two quantities of heat $Mc(T_1 - T_0)$ and $m_1 r_1$ contained in $Q_1$ must be separately considered. In order to execute the integration extending over the first quantity, we have but to give to the element of heat $dQ$ the form $Mc dT$, and this part of the integral is at once expressed by

$$Mc \int_{T_0}^{T_1} \frac{dT}{T} = Mc \log \frac{T_1}{T_0}.$$

During the communication of the latter quantity of heat, the temperature is constant and equal to $T_1$, and consequently the part of the integral referring to this quantity is simply

$$\frac{m_1 r_1}{T_1}.$$

By substituting these values, the foregoing expression for $W'$ becomes

$$W' = \frac{1}{A}\left[ m_1 r_1 + Mc(T_1 - T_0) - T_0\left( \frac{m_1 r_1}{T_1} + Mc \log \frac{T_1}{T_0} \right) \right]$$

$$= \frac{1}{A}\left[ m_1 r_1 \frac{T_1 - T_0}{T_1} + Mc\left( T_1 - T_0 + T_0 \log \frac{T_1}{T_0} \right) \right];$$

and this is the same expression as that contained in equation (XI), which was before obtained by the successive determination of the several quantities of work done during the circular process.

24. From this it follows, *that if the temperatures at which the matter manifesting the action of heat receives heat from the source of heat, or imparts heat to some external object, are considered as previously given,* then the steam-engine, under the conditions made in deducing the equation (XI), is a *perfect* machine; that

is to say, for a certain amount of imparted heat it furnishes as much work as, according to the mechanical theory of heat, is possible at those temperatures.

It is otherwise, however, *when those temperatures, instead of being given, are also considered as a variable element, to be taken into consideration in judging the machine.*

One uncompensated transformation not included in N, which, with respect to the applicability of heat, causes a great loss, arises from the fact that the liquid, during the processes of heating and evaporation, has far lower temperatures than the fire, and consequently the heat which is imparted to it must pass from a higher to a lower temperature. The amount of work which can be produced by the steam-engine from the quantity of heat $m_1 r_1 + Mc(T_1 - T_0) = Q_1$, is, as may be seen from equation (27), somewhat smaller than

$$\frac{Q_1}{A} \cdot \frac{T_1 - T_0}{T_1}.$$

If, on the contrary, we could impart the same quantity of heat $Q_1$ to a changeable body at the temperature of the fire, which may be $T'$, whilst the temperature during the abstraction of heat remained $T_0$, as before, then by equation (4) the greatest possible amount of work to be gained in such a case would be

$$\frac{Q_1}{A} \cdot \frac{T' - T_0}{T'}.$$

In order to compare the values of these expressions in a few examples, let the temperature $t_0$ of the condenser be fixed at 50° C., and for the boiler let us assume the temperatures 110°, 150°, and 180° C., of which the two first correspond approximately to the low- and the ordinary high-pressure machines respectively, and the last may be considered as the limit of the temperatures hitherto employed in steam-engines. In these cases the fraction dependent upon the temperatures has the following values:—

| $t_1$. | 110° | 150° | 180° |
|---|---|---|---|
| $\dfrac{T_1 - T_0}{T_1}$. | 0·157 | 0·236 | 0·287 |

whereas the corresponding value for the temperature of the fire $t'$, assuming the latter to be only 1000° C., is 0·746.

25. We may here easily discern, what has already been expressed by S. Carnot and several other authors, that in order to render machines driven by heat more efficient, attention must be particularly directed towards the enlargement of the interval of temperature between $T_1$ and $T_0$.

For instance, machines driven by heated air will only attain a decided advantage over steam-engines when a method is found of allowing them to work at a far higher temperature than steam-engines, in consequence of the danger of explosions, can bear. The same advantage, however, could be attained with over-heated vapour; for as soon as the vapour is separated from the liquid, it is just as safe to heat it further as to heat a permanent gas. Machines employing vapour in this condition may possess many of the advantages of the steam-engine besides those of air-machines, so that a practical improvement may sooner be expected from these than from air-machines.

In the machines above mentioned, where, besides water, a second more vaporizable substance was employed, the interval $T_1 - T_0$ is increased by lowering $T_0$. It has already been suggested, that this interval might be increased in a similar manner on the upper side, by the addition of a third liquid less vaporizable than water. In such a case the fire would be immediately applied to the evaporation of the least vaporizable of the three substances, the condensation of this to the evaporation of the second, and the condensation of the second to the evaporation of the third. Theoretically, there is no doubt that such a combination would be advantageous; the practical difficulties, however, which would have to be overcome in realizing such a scheme cannot of course be predicted.

26. Besides the above-mentioned defect, arising out of the very nature of our ordinary steam-engines, these machines suffer from many other imperfections, which may be ascribed more immediately to defective construction.

One of these has already been considered in the foregoing development, and allowed for in equation (X), that is to say, the expansion cannot be continued nearly far enough to allow the vapour in the cylinder to reach the temperature of the condenser. If, for example, we assume the temperature of the boiler to be 150°, and that of the condenser to be 50°, then the Table in § 16 shows, that, for the above purpose, the expansion must be prolonged to twenty-six times the original volume; whereas in practice, owing to many inconveniences attending great expansions, three or four, and at most ten times the original volume is attained.

Two other imperfections, however, are expressly excluded in the foregoing: these are, first, *that the pressure of the vapour in one part of the cylinder is smaller than in the boiler, and in the other part greater than in the condenser*; and secondly, *the presence of vicious space.*

We must consequently extend our former considerations so as to include these imperfections.

XXXIV. *On the Demonstration of* Fresnel's *Formulas for Reflected and Refracted Light.*—No. III. *By the* Rev. BADEN POWELL, *M.A., F.R.S. &c., Savilian Professor of Geometry in the University of Oxford*\*.

1. IN two former papers (Phil. Mag. July and August 1856), especially in the last paper, I have shown that, on received principles, the *original* formulas of Fresnel are apparently necessary for the application of theory to certain experimental results, to the exclusion of some *newer* modifications, though deduced on more systematic theoretical grounds.

In opposition to this, however, another view has been suggested (as there mentioned), which, if true, would set aside all the reasoning hitherto adopted on the subject, but which to me seems open to great doubt in itself.

It is, however, clear that Fresnel's original formulas cannot *both* be deduced on any common principle hitherto proposed, it being, as far as yet appears, necessary to assume a separate hypothesis for each of the two cases, and these not apparently reconcileable with each other.

2. If the considerations I have adduced in my second paper (§§ 24, 25) be regarded as well founded, it becomes highly important to find some mode of deducing both Fresnel's original formulas on a common principle.

But whether the arguments I have advanced be thought valid or not, it must still be allowed, on all hands, to be a matter of some interest if possible to suggest a proof free from the objections mentioned.

Since writing those papers, it has appeared to me that this may be effected, provided the following considerations be admitted relative to the law of equivalent vibrations, which (as before hinted) appears to be the doubtful element in the former investigations.

3. In the case (α) of vibrations *perpendicular* to the plane of incidence, and where the incident, reflected and refracted, vibrations are all parallel to each other and to the surface, there is no difficulty. Here there is no geometrical construction from which to find the relation of the amplitudes. In this case the proof of equivalence depends directly on mechanical considerations alone, agreeably to the reasoning referred to before (first paper, § 26). Here $h$, $h'$, and $h_{\prime}$ being the mechanical values of the amplitudes, we have simply for the law of equivalence,

$$h_{\prime} = h + h'.$$

4. In the case (β) of vibrations *parallel* to the plane of inci-

dence, we have the obvious geometrical construction of the triangle formed by the known directions of the rays, and consequently of the amplitudes at right angles to them, and in the same plane (first paper, § 23). In this triangle, the angles being $(i-r)$, $(i+r)$, and $(\pi-2i)$, the ratios of the sides will be (as before)

$$\frac{h'}{h} = \frac{\sin(i-r)}{\sin(i+r)}, \quad \frac{h_i}{h} = \frac{\sin 2i}{\sin(i+r)}.$$

5. On Maccullagh's theory of *equal* densities, it will be remembered that this construction represents the mechanical equivalence, and the sides thus give the values of the amplitudes.

The close agreement of these values with those for the amplitudes in case $(\alpha)$ deduced on the hypothesis of *increased* density, —to which no such construction can apply,—has been already remarked (first paper, §§ 25, 41).

But these values cannot be those of the amplitudes in case $(\beta)$ on the hypothesis of increased density, since experimental results essentially require a different relation in this case from that in case $(\alpha)$.

6. From the triangle, however, we have this obvious and simple relation of equivalence,

$$h_i = h \cos(i-r) + h' \cos(i+r) ;$$

or expressing these components parallel to $h_i$ by $(h)$ $(h')$, we have

$$h_i = (h) + (h').$$
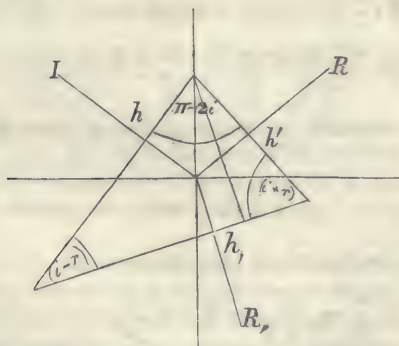


But in this construction, it must be borne in mind that if we assume the side $h_i$ as the value of the refracted amplitude, it is represented geometrically on the same scale with the other sides by which we measure the components; and for its actual value it is necessary to take into account its diminution in the more retarding medium in the ratio of the refractive index; or if $(h_i)$

be this physical value, we must take

$$(h_,) = h_, \frac{1}{\mu} = \big( (h) + (h') \big) \frac{1}{\mu}.$$

Thus both cases of equivalence may be included in one expression ($\theta$ being the inclination of the plane of vibration to that of incidence),

$$\big( (h) + (h') \big) \Big( \sin\theta + \cos\theta \, \frac{1}{\mu} \Big) = (h_,),$$

where $\theta = 90°$ gives case ($\alpha$), and $\theta = 0°$ gives case ($\beta$).

7. If this be admitted as the true expression of the law of equivalence, we can deduce *both Fresnel's original formulas* from *this* law of equivalence combined with that of *vis viva*, viz.

$$(h^2 - h'^2)m = h_,^2 m_,.$$

($\alpha$) For vibrations *perpendicular* to the plane of incidence, the law of equivalence, $(h + h') = h_,$, combined with the law of *vis viva*, gives

$$(h - h') \frac{m}{m_,} = \frac{h_,^2}{h + h_,} = h + h_,.$$

But on the hypothesis of increased density or retardation, as before (first paper, § 17),

$$\frac{m}{m_,} = \frac{\sin r \cos i}{\sin i \cos r}$$

$$(h - h') \sin r \cos i = (h + h') \sin i \cos r,$$

or

$$h(\sin r \cos i - \sin i \cos r) = h'(\sin r \cos i + \sin i \cos r)$$

$$\frac{h'}{h} = \frac{-\big( \sin (i - r) \big)}{\sin (i + r)}, \quad \frac{h_,}{h} = \frac{2 \sin r \cos i}{\sin (i + r)},$$

8. ($\beta$) For vibrations *parallel* to the plane of incidence, the law of equivalence (writing $k k' k_,$ instead of $(h) \ (h') \ (h_,)$),

$$(k + k') \frac{1}{\mu} = k_,,$$

combined with the law of *vis viva*, gives

$$(k - k') \frac{m}{m_,} = \frac{k_,^2}{k + k'} = (k + k') \frac{1}{\mu^2}.$$

But

$$\frac{m}{m_,} \mu^2 = \frac{\sin r \cos i}{\sin i \cos r} \cdot \frac{\sin^2 i}{\sin^2 r} = \frac{\sin 2i}{\sin 2r}$$

$$(k - k') \sin 2i = (k + k') \sin 2r,$$

or

$$k(\sin 2i - \sin 2r) = k'(\sin 2i + \sin 2r)$$

$$\frac{k'}{k} = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r} = \frac{\tan (i-r)}{\tan (i+r)}$$

$$k_{_l} = (k+k') \frac{1}{\mu} 2 \sin 2i \frac{1}{\mu} = 4 \sin r \cos i$$

$$\frac{k_{_l}}{k} = \frac{4 \sin r \cos i}{\sin 2i + \sin 2r} = \left(1 - \frac{\tan (i-r)}{\tan (i+r)}\right) \frac{\cos i}{\cos r}.$$

9. These amplitudes are portions of the sides of the triangle taken in the ratio of the components; and if in that ratio before given we substitute the value of $\frac{h'}{h}$, we find

$$\frac{(h')}{(h)} = \frac{\sin (i-r) \cos (i+r)}{\sin (i+r) \cos (i-r)} = \frac{\sin 2i - \sin 2r}{\sin 2i + \sin 2r},$$

which coincides with the value just deduced on the principle of the *vis viva*.

Thus for both cases we have Fresnel's *original* formulas.

10. It is also worthy of remark, that as the investigation of case ($\alpha$) is obviously equivalent to the direct application of the formula for impact, viz.

$$v = \frac{m - m_{_l}}{m + m_{_l}}, \quad v_{_l} = \frac{2m}{m + m_{_l}},$$

so on the principle here adopted for case ($\beta$), it is easily seen that the same is true, if here, instead of $m\ m_{_l}$, we take (as above (8))

$$(m) = \frac{m \sin i}{\sin r} = \sin 2i \text{ and } (m_{_l}) = \frac{m_{_l} \sin r}{\sin i} = \sin 2r.$$

The result is the same as would be obtained directly from the equations

$$v = \frac{(m) - (m_{_l})}{(m) + (m_{_l})}, \quad v_{_l} = \frac{2(m)}{(m) + (m_{_l})}.$$

11. It is also remarkable that this result is the same as if in case ($\alpha$) we had supposed increased density, but in case ($\beta$) equal densities.

It may also be observed, that in the investigations of Fresnel and Maccullagh the analogy of impact is referred to only in the case of vibrations *perpendicular* to the plane of incidence, where the mechanical analogy is not apparent; whereas in the present investigation we see that that analogy applies also to the case of vibrations *parallel* to the plane of incidence, which is the actual case in ordinary mechanical impact.

---

*Erratum in first Paper.*

§ 37, line 2, *dele* " or direction of vibrations."

## XXXV. *On Rubian and its Products of Decomposition.*
### *By* Edward Schunck, *F.R.S.*

*ACTION of Chlorine on Rubian.*—If a current of chlorine gas be passed through a watery solution of rubian, the latter begins immediately to deposit flocks of a lemon-yellow or orange colour. These flocks continue to be formed as long as the solution retains any portion of its yellow colour. When the action is completed the liquid appears colourless. The flocks, the quantity of which is considerable compared with that of the rubian employed, consist almost entirely of one substance, which I shall call *Chlororubian*, though this name is not perfectly appropiate, since it is not formed from rubian simply by the substitution of hydrogen by chlorine. If these flocks, after being collected on a filter and washed until all the acid and chlorine are removed, be treated with a little cold alcohol, the latter dissolves a small quantity of a substance, which after the evaporation of the alcohol is left as a yellow or yellowish-brown resinous residue. This substance melts at the temperature of boiling water; it contains chlorine, and dissolves in caustic alkalies with a dirty purple colour. The chlororubian may be purified by simply dissolving it in boiling alcohol. It crystallizes on the solution cooling in small orange-coloured needles, which increase very much in quantity after standing for several hours. The acid liquid, filtered from the yellow flocks formed by the action of chlorine, contains sugar, which may be obtained by neutralizing the acid with carbonate of lead, filtering, evaporating the liquid to a small volume, decolorizing with animal charcoal, filtering, evaporating to dryness, and treating the residue with alcohol. The alcohol after filtration and evaporation leaves a yellow syrup having all the properties of sugar, as usually obtained by the decomposition of rubian. Chlororubian may be prepared as well from impure as from pure rubian. It is only necessary to extract madder with boiling water, add sugar of lead to the extract, add ammonia to the liquid filtered from the precipitate, decompose the red precipitate produced by ammonia with sulphuric acid, and pass chlorine gas through the filtered liquid. The first portions of chlorine generally produce a dirty yellow flocculent precipitate, which, being separated by filtration, is found to consist of the resinous easily fusible substance just mentioned. On passing chlorine through the filtered liquid, pure yellow flocks of chlororubian are precipitated, which are purified as before by crystallization from boiling alcohol.

Chlororubian has the following properties. After crystallization from alcohol and drying, it forms a mass of a light orange

colour, consisting of small crystalline needles. It has no per-
ceptible taste at first, but on chewing it for some time it pro-
duces a slightly bitter taste. When heated on platinum it melts
and burns with a smoky flame slightly tinged with green, and
leaves a considerable quantity of charcoal. On being heated in
a tube it melts to a brown liquid, and gives fumes which condense
on the colder parts of the tube to a white crystalline sublimate,
consisting of star-shaped masses, while much carbonaceous resi-
due is left. On being treated with boiling water chlororubian
dissolves in considerable quantity, forming a yellow solution,
which on cooling deposits a great part of the substance, not in
crystals, but in amorphous masses consisting of spherical grains.
The boiling alcoholic solution, if very concentrated, also deposits
part of the substance on cooling in amorphous, spherical, trans-
lucent grains, which have the appearance of drops of oil, but by
redissolving these in fresh alcohol, crystals of the usual appear-
ance are obtained. The alcoholic solution does not redden litmus
paper in the least. The watery solution gives no precipitate with
nitrate of silver, but if chlororubian be treated with boiling nitric
acid, it is decomposed with an evolution of nitrous acid, forming
a colourless solution, in which nitrate of silver produces a pre-
cipitate of chloride of silver. The action of sulphuric and muri-
atic acids, caustic alkalies and chlorine on chlororubian, I shall
treat of presently. Chlororubian dissolves in boiling solutions
of the carbonates of potash, soda and ammonia, forming blood-
red solutions, which deposit nothing on cooling. Baryta water
imparts to the watery solution a deep red colour, and on boiling
dark red flocks are deposited, while the liquid becomes almost
colourless. Lime water turns the watery solution red without
producing any precipitate, but the ammoniacal solution gives
with chloride of calcium a light red flocculent precipitate, while
the supernatant liquid becomes colourless. The watery solution
gives no precipitate with the acetates of alumina and peroxide
of iron. On being treated with a boiling solution of perchloride
of iron, chlororubian dissolves with a brownish-yellow colour,
which after some time becomes dark brown, while a black powder
is deposited. The alcoholic solution of chlororubian does not
change on being mixed with an alcoholic solution of acetate of
lead, but the watery solution gives with basic acetate of lead a
light red precipitate, the liquid retaining a reddish colour. The
alcoholic solution gives no precipitate with acetate of copper.
An alkaline solution of chlororubian reduces chloride of gold to
the metallic state, even in the cold. Chlororubian produces no
effect on mordants, on trying to dye with it in the usual manner.

On submitting chlororubian to analysis the following results
were obtained :—

I. 0·4100 grm., dried in the water bath and burnt with chromate of lead, gave 0·7690 carbonic acid and 0·1860 water.

0·4760 grm., burnt with lime, gave 0·1260 chloride of silver.

II. 0·4780 grm. of the same, recrystallized from boiling alcohol, gave 0·8950 carbonic acid and 0·2100 water.

III. 0·5470 grm. of another preparation gave 1·0270 carbonic acid and 0·2400 water.

0·7720 grm., burnt with lime, gave 0·2015 chloride of silver.

IV. 0·4425 grm. of the same preparation as the last gave 0·8320 carbonic acid and 0·1960 water.

0·5450 grm., on decomposition with fuming nitric acid to which a little nitrate of silver was added, gave 0·1380 chloride of silver.

V. 0·4975 grm. of a new preparation gave 0·9330 carbonic acid and 0·2240 water.

0·8580 grm., burnt with lime, gave 0·2180 chloride of silver.

VI. 0·5320 grm. gave 1·0000 carbonic acid and 0·2380 water.

0·9370 grm., burnt with lime, gave 0·2450 chloride of silver.

These numbers correspond in 100 parts to—

|          | I. | II. | III. | IV. | V. | VI. |
|----------|------|------|------|------|------|------|
| Carbon   | 51·15 | 51·06 | 51·20 | 51·27 | 51·14 | 51·26 |
| Hydrogen | 5·04 | 4·88 | 4·87 | 4·92 | 5·00 | 4·97 |
| Chlorine | 6·54 | „ | 6·45 | 6·26 | 6·28 | 6·46 |
| Oxygen   | 37·27 | „ | 37·48 | 37·55 | 37·58 | 37·31 |

There are several formulæ which give a composition in 100 parts agreeing tolerably well with these numbers, but only one which at the same time explains the manner in which chlororubian is formed. This formula is $C^{44} H^{27} ClO^{24}$, which gives the following composition:—

|          |  | Eqs. |  |  |
|----------|--|------|-----|-------|
| Carbon   | . | 44 | 264 | 50·92 |
| Hydrogen | . | 27 | 27 | 5·20 |
| Chlorine | . | 1 | 35·4 | 6·82 |
| Oxygen   | . | 24 | 192 | 37·06 |
|          |  |  | 518·4 | 100·00 |

Assuming this formula to be correct, then rubian, when acted on by chlorine, loses 1 equivalent of hydrogen, which is replaced by chlorine, at the same time taking up 6 equivalents of water and splitting up into chlororubian and sugar, as will be seen from the following equation:—

$$C^{56} H^{34} O^{30} + 6HO + 2Cl = C^{44} H^{27} ClO^{24} + C^{12} H^{12} O^{12} + ClH.$$
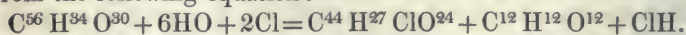
*Action of Acids on Chlororubian.*—If chlororubian be treated with boiling dilute sulphuric or muriatic acid, it first dissolves to a yellow liquid, but on continuing to boil, the solution suddenly becomes milky and deposits a large quantity of yellow

crystalline flocks. The filtered liquid is almost colourless, and contains sugar. The flocks consist entirely of a body, to which, as it has the composition of rubiadine in which 1 equivalent of hydrogen is substituted by chlorine, I shall give the name of *Chlororubiadine.* It is purified by collecting the flocks on a filter, washing them with water, and dissolving them in boiling alcohol, which on cooling and standing, deposits yellow shining crystals, which are larger than those of chlororubian.

Chlororubiadine has the following properties. When crystallized from alcohol and dried, it has the appearance of a yellow mass, consisting of small shining crystalline needles and scales. When heated on platinum it melts to a brown liquid, and then burns with a yellow flame bordered with green, leaving much carbonaceous residue. When heated in a tube it melts and gives penetrating fumes, smelling of muriatic acid, and forming on the colder parts of the tube a sublimate which is at first oily but soon becomes crystalline. It is insoluble in boiling water. Dilute nitric acid does not affect it, even on boiling. Nitric acid of sp. gr. 1·52, however, dissolves it, even in the cold, forming a dark orange-coloured solution. If nitrate of silver be added to this solution, no precipitate is produced; but if the solution be boiled, an evolution of nitrous acid takes place, the solution becomes turbid, and gives a copious deposit of chloride of silver. Concentrated sulphuric acid dissolves it in the cold, forming an orange-coloured solution, from which it is precipitated again by water in bright yellow flocks. If the solution in the acid be boiled its colour changes to a deep purple, without much sulphurous acid being evolved, a small quantity of a white crystalline sublimate making its appearance after some time on the sides of the vessel near the surface of the liquid. Chlororubiadine is easily dissolved by caustic soda with a purplish-red colour, and by ammonia and the carbonates of potash, soda and ammonia, with a blood-red colour. The ammoniacal solution loses its ammonia on evaporation, and leaves the substance behind as a bright yellow residue. On adding chloride of barium to the ammoniacal solution, the baryta compound crystallizes out on standing in long needles, arranged in large fan-shaped or star-shaped masses of a beautiful red colour, while the liquid becomes almost colourless. The baryta compound, when treated with boiling water, only dissolves in part, some chlororubiadine being left undissolved. If a current of carbonic acid be passed through the filtered solution, the whole of the chlororubiadine is precipitated in yellow flocks, the liquid becoming colourless. The ammoniacal solution gives with chloride of calcium, after some time, a dark red uncrystalline deposit, and also loses its colour. A boiling solution of perchloride of

iron does not dissolve chlororubiadine, nor does the colour of the solution change during boiling. The alcoholic solution of chlororubiadine reddens litmus paper. The solution gives no precipitate with acetate of lead, not even on the addition of ammonia, but with acetate of copper it gives after some time a copious light brown precipitate. Acetate of alumina and peracetate of iron produce no change in the alcoholic solution. An alkaline solution of chlororubiadine reduces chloride of gold to the metallic state.

The composition of chlororubiadine was determined by the following analyses :—

I. 0·5650 grm., dried at 100° C. and burnt with chromate of lead, gave 1·2630 carbonic acid and 0·2130 water.

0·4850 grm., burnt with lime, gave 0·2200 chloride of silver.

II. 0·4620 grm. of a second preparation gave 1·0260 carbonic acid and 0·1760 water.

III. 0·5475 grm., made from the last by dissolving it in ammonia, adding chloride of barium, filtering the liquid from the dark flocks which were precipitated, allowing the baryta compound to crystallize, decomposing it with muriatic acid, and crystallizing what was left by the acid from boiling alcohol, gave 1·2280 carbonic acid and 0·2100 water.

0·6260 grm. gave 0·2775 chloride of silver.

IV. 0·4980 grm. of another preparation, obtained like the last from the baryta compound, gave 1·1130 carbonic acid and 0·1905 water.

0·7680 grm. gave 0·3455 chloride of silver.

Hence the following composition may be deduced :—

|          | Eqs. |       | Calculated. | I.     | II.    | III.   | IV.    |
|----------|------|-------|-------------|--------|--------|--------|--------|
| Carbon   | 32   | 192   | 61·65       | 60·96  | 60·56  | 61·17  | 60·95  |
| Hydrogen | 12   | 12    | 3·85        | 4·18   | 4·23   | 4·26   | 4·25   |
| Chlorine | 1    | 35·4  | 11·36       | 11·21  | „      | 10·95  | 11·10  |
| Oxygen   | 9    | 72    | 23·14       | 23·65  | „      | 23·62  | 23·70  |
|          |      | 311·4 | 100·00      | 100·00 |        | 100·00 | 100·00 |

The baryta compound I found to have no very simple composition. It was prepared, as above described, by dissolving crystallized chlororubiadine in ammonia, adding chloride of barium, filtering from a few flocks that were precipitated, and allowing to crystallize in an air-tight flask, filtering, washing with water, and drying *in vacuo.*

0·8370 grm. of the crystals lost, on being heated for some hours in the water-bath, 0·0690 water = 8·24 per cent.

0·5770 grm. of the substance thus dried gave, when burnt with chromate of lead, 1·0965 carbonic acid and 0·1790 water.

0·5660 grm. gave 0·1350 sulphate of baryta.

These numbers lead to the formula $4C^{32} H^{12} ClO^9 + 3BaO$, as

the following comparison between the theoretical composition and the experimental results will show :—

|  | Eqs. |  | Calculated. | Found. |
|---|---|---|---|---|
| Carbon . . | 128 | 768 | 52·05 | 51·82 |
| Hydrogen . | 48 | 48 | 3·25 | 3·44 |
| Chlorine . . | 4 | 141·6 | 9·59 | ,, |
| Oxygen . . | 36 | 288 | 19·54 | ,, |
| Baryta . . | 3 | 229·8 | 15·57 | 15·65 |
|  |  | 1475·4 | 100·00 |  |

The sugar which is formed from chlororubian together with chlororubiadine may be obtained in a crystallized state, which is not the case with the different specimens of sugar derived from the other processes of decomposition to which I have subjected rubian. If sulphuric acid be employed for the decomposition of chlororubian, and the acid after filtration of the flocks of chlororubiadine be neutralized with carbonate of lead, the filtered liquid yields on evaporation a sweet syrup. If this syrup be treated with alcohol, a part of it dissolves with a yellow colour. If the alcoholic solution, after separation from the insoluble part, be mixed with several times its volume of æther, it becomes milky and deposits again a yellow syrup, which after standing some time becomes filled with small yellowish crystals, so as almost to form a solid mass. This mass is pressed between blotting-paper, in order to remove the mother-liquor, and the crystals are dissolved in boiling alcohol, to which a little animal charcoal is added. The filtered solution on evaporation gives a syrup, which is soon converted into a mass of white crystals. These crystals have the properties and composition of crystallized grape-sugar.

0·5015 grm. of the crystals, dried *in vacuo*, gave 0·6830 carbonic acid and 0·3380 water.

These numbers lead to the following composition :—

|  | Eqs. |  | Calculated. | Found. |
|---|---|---|---|---|
| Carbon . . | 12 | 72 | 36·36 | 37·14 |
| Hydrogen . | 14 | 14 | 7·07 | 7·48 |
| Oxygen . . | 14 | 112 | 56·57 | 55·38 |
|  |  | 198 | 100·00 | 100·00 |

It will now admit, I think, of little doubt, that the uncrystallizable sugar obtained from rubian in other processes of decomposition is merely modified grape-sugar.

The formation of chlororubiadine and sugar from chlororubian is a very simple process. The latter loses three equivalents of water and splits up into chlororubiadine and sugar, as will be evident from the following equation :—

$$C^{44} H^{27} ClO^{24} = C^{32} H^{12} ClO^9 + C^{12} H^{12} O^{12} + 3HO.$$
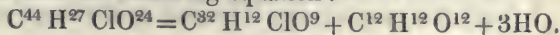
I have adopted the name of chlororubiadine, under the assumption that the true formula for rubiadine is $C^{32} H^{13} O^9$. Nevertheless I have not succeeded in converting the latter into chlororubiadine by means of chlorine, nor in substituting the chlorine in chlororubiadine by hydrogen and thus forming rubiadine. If chlororubiadine be suspended in water to which an amalgam of potassium (1 part of potassium to 100 mercury) is added, it dissolves with a dirty red colour without much hydrogen being evolved. The liquid gives a greenish-yellow flocculent precipitate on the addition of nitric acid. This precipitate contains no chlorine, but it does not contain any rubiadine, since it gives, after being dried and heated between two watch-glasses, none of the crystalline sublimate characteristic of rubiadine. The liquid filtered from this precipitate gives an abundant precipitate with nitrate of silver.

Hydrosulphate of ammonia dissolves chlororubiadine, forming a red solution, which on standing becomes of a fine purple, and after some hours brownish-red. If nitric acid be added to the solution as soon as it has acquired a purple colour, an orange-coloured flocculent precipitate falls. This precipitate is free both from sulphur and chlorine, the chlorine of the chlororubiadine being found in the filtered liquid, but it contains no rubiadine. It is only partly soluble in boiling alcohol, but dissolves easily in boiling nitric acid, the solution depositing on standing some long dark yellow sword-shaped crystals.

If crystallized rubiadine, obtained by the decomposition of rubihydran with acid, be dissolved in caustic alkali and reprecipitated with acid, and if the precipitated flocks after filtering and washing be suspended in water, and a current of chlorine gas be passed through the liquid, the flocks become somewhat paler in colour. If they be now collected on a filter and washed with water and then treated with cold alcohol, the greater part dissolves, leaving undissolved a small quantity of a white powder, which has all the properties of a body which I shall describe presently, and which is formed by the action of chlorine on chlororubian. If the alcoholic solution be evaporated to dryness and a little cold alcohol be again added to the residue, the alcohol again dissolves a great part, leaving undissolved a yellowish-green granular powder, which resembles but is not identical with chlororubiadine. The alcoholic solution leaves on evaporation a brown, transparent, resinous substance. This substance contains a large quantity of chlorine. It dissolves in caustic soda with a brown colour, and is reprecipitated by acids in yellow flocks, which melt in the boiling liquid to brown oily drops. I found it to contain the following quantities of carbon and hydrogen :—

0·3090 grm., dried at 100° C. and burnt with chromate of lead, gave 0·6090 carbonic acid and 0·1060 water, corresponding in 100 parts to—

Carbon . . . . 53·75
Hydrogen. . . . 3·81

The formula $C^{32} H^{13} Cl^2 O^{10} = C^{32} H^{12} Cl^2 O^9 + HO$ requires in 100 parts—

Carbon . . . . 53·96
Hydrogen . . . . 3·65
Chlorine . . . . 19·89
Oxygen . . . . 22·50

I may mention by the way, that the brown resinous substance which, as I stated above, is formed in small quantities along with chlororubian by the action of chlorine on rubian, is very similar both in properties and composition to this substance. It was prepared simply by passing chlorine through a solution of rubian, collecting the yellow flocks which were formed in the first instance separately, washing them with water, and treating with cold alcohol. The filtered solution was evaporated to dryness, when it left a resinous substance resembling rubiretine.

0·4130 grm. of this substance, dried in the water-bath, gave 0·8200 carbonic acid and 0·1500 water.

0·3010 grm., burnt with lime, gave 0·2160 chloride of silver.

These numbers correspond in 100 parts to—

Carbon . . . . 54·14
Hydrogen . . . . 4·03
Chlorine . . . . 17·74
Oxygen . . . . 24·09

By the action of chlorine on chlororubiadine, a body very similar to these but differing in composition is formed. If finely pounded chlororubiadine be suspended in water and a stream of chlorine gas be passed through the liquid for some time, the powder becomes lighter in colour, but not white. If it now be collected on a filter, washed with water, and treated with boiling alcohol, it dissolves in the latter, forming a yellow solution, which deposits nothing on cooling, but on evaporation leaves a transparent dark yellow amorphous substance like resin, which remains soft for a long time, and only becomes hard and brittle after being heated in the water-bath for some time. This substance, when heated on platinum, burns with a yellow flame, leaving much charcoal. When heated in a tube, it melts and gives acid fumes and a yellow oily sublimate, in which on cooling some white crystals make their appearance. The alcoholic solution gives no precipitate with nitrate of silver, but

the substance on being treated with boiling nitric acid is dissolved and decomposed, and nitrate of silver now gives an abundant precipitate. It is soluble in concentrated sulphuric acid with a brown colour, but on boiling the solution no sulphurous acid is evolved. Caustic soda dissolves it easily with a brown colour. On being analysed it yielded the following results :—

0·3910 grm., dried in the water-bath, gave 0·6675 carbonic acid and 0·1100 water.

0·4055 grm. gave 0·4990 chloride of silver.

In 100 parts it contained therefore—

| | | | | |
|---|---|---|---|---|
| Carbon | . | . | . | . | 46·55 |
| Hydrogen. | . | . | . | 3·12 |
| Chlorine . | . | . | . | 30·42 |
| Oxygen | . | . | . | . | 19·91 |

Since the carbon here is to the chlorine as $32 \text{ C} : 3\frac{1}{2} \text{ Cl}$, this substance must either have been a mixture, or it must have lost chlorine during the process of drying.

*Action of Caustic Alkalies on Chlororubian.*—The action of alkalies on chlororubian differs essentially from that of acids. The chlorine in chlororubian is so loosely combined that the affinity of the alkaline metal is sufficient to remove it, and hence all the organic products of decomposition formed by the alkali are free from chlorine.

If chlororubian be treated with a solution of caustic soda it dissolves easily, forming a red solution. If this solution be heated for some time, it deposits a quantity of dark reddish-brown flocks. When these flocks cease to be formed, the liquid, which is still red, is filtered, the flocks are washed with water until the excess of soda is removed, and they are then treated with boiling muriatic acid, by which their colour is changed to yellowish-brown. After being collected on a filter and washed with water, they are then treated with boiling alcohol, in which they are but little soluble, placed on a filter, washed with alcohol and dried. After drying, there is obtained a yellowish-brown powder which has the following properties. It is almost insoluble both in alcohol and in caustic alkalies, though the latter impart to it a dark reddish-brown colour. It is not dissolved by hydrosulphate of ammonia. It contains no chlorine. When heated in a tube it gives a yellow crystalline sublimate, which dissolves easily in caustic alkalies. Its composition was determined by the following analyses :—

I. 0·3980 grm., dried at 100° C. and burnt with chromate of lead, gave 1·0330 carbonic acid and 0·1390 water.

II. 0·3910 grm. gave 1·0150 carbonic acid and 0·1370 water.

III. 0·4000 grm. of a new preparation gave 1·0350 carbonic acid and 0·1435 water.

In 100 parts it contained therefore—

|  | I. | II. | III. |
|---|---|---|---|
| Carbon | 70·78 | 70·79 | 70·56 |
| Hydrogen | 3·88 | 3·89 | 3·98 |
| Oxygen | 25·34 | 25·32 | 25·46 |

It is doubtful whether this body contains 44 or 32 equivalents of carbon. The formulæ $C^{44} H^{14} O^{12}$, $C^{32} H^{10} O^8$ and $C^{32} H^{11} O^9$ require in 100 parts respectively the following amounts of the three constituents :—

|  | $C^{44} H^{14} O^{12}$. | $C^{32} H^{10} O^8$. | $C^{32} H^{11} O^9$. |
|---|---|---|---|
| Carbon | 70·58 | 72·18 | 69·81 |
| Hydrogen | 3·74 | 3·75 | 4·00 |
| Oxygen | 25·68 | 24·07 | 26·19 |

If the first formula be adopted, then this substance is formed from chlororubian by the latter losing its chlorine, which combines with sodium and is replaced by oxygen, while 13 equivalents of water are eliminated, in accordance with the following equation :—

$$C^{44} H^{27} ClO^{24} + NaO = C^{44} H^{14} O^{12} + NaCl + 13HO.$$
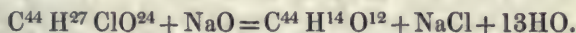
If one of the two latter formulæ be adopted, then chlororubian first splits up into chlororubiadine and sugar, and the former then loses its chlorine which is replaced by oxygen, while either five or four equivalents of water separate, as will be seen from the following equations :—

$$C^{44} H^{27} ClO^{24} + NaO = C^{32} H^{10} O^8 + C^{12} H^{12} O^{12} + NaCl + 5HO,$$
$$C^{44} H^{27} ClO^{24} + NaO = C^{32} H^{11} O^9 + C^{12} H^{12} O^{12} + NaCl + 4HO.$$
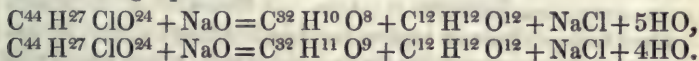
The first formula agrees best with the results of analysis. Nevertheless, it seems improbable that chlororubian, when subjected to the action of so potent an agent as caustic soda, should not, in the first instance at least, be decomposed in the same manner as with strong acids. On the other hand, if this were the case, chlororubiadine should, by the action of caustic alkalies, be converted into the same body as chlororubian, which is not the case. If chlororubiadine be treated with caustic soda in the same way as chlororubian, it first dissolves with a purple colour, but the solution on boiling slowly deposits reddish-brown flocks and loses the greatest part of its purple colour. The flocks on being treated with boiling muriatic acid, acquire an orange colour, and after being collected on a filter and washed, are found to be almost insoluble in boiling alcohol; but notwithstanding their resemblance to the body formed from chlororubian, they differ from the latter in containing a large quantity of chlorine, only a small quantity of the latter having been abstracted by the alkali.

0·3610 grm. of this substance, dried at 100° C., gave 0·8620 carbonic acid and 0·1060 water.

0·2920 grm. gave 0·1110 chloride of silver.

In 100 parts it contained therefore—

|  |  |
|---|---|
| Carbon | 65·12 |
| Hydrogen | 3·26 |
| Chlorine | 9·39 |
| Oxygen | 22·23 |

Should the body formed from chlororubian by caustic alkalies be found to contain 44 equivalents of carbon, the most appropriate name for it would be *Oxyrubian*.

The liquid filtered from this body is still red. On adding to it sulphuric acid, a yellowish-brown flocculent precipitate falls. This precipitate consists of several bodies. If after being filtered and washed it be treated with boiling alcohol, a part dissolves, leaving undissolved a dark brown substance, which after drying becomes black. This substance is doubtless a product of the decomposition of sugar, as it has the same properties and very nearly the same composition as the body, insoluble in alcohol, which I obtained by the decomposition of rubian with caustic soda*.

0·1760 grm. of this substance gave 0·4360 carbonic acid and 0·0650 water, corresponding in 100 parts to—

|  |  |
|---|---|
| Carbon | 67·56 |
| Hydrogen | 4·10 |
| Oxygen | 28·34 |

On adding acetate of lead to the liquid filtered from this precipitate, a brown precipitate falls, which after being filtered off, washed with alcohol and decomposed with boiling muriatic acid, yields brown flocks. These, on being dried and treated with cold alcohol, yield to the latter a body resembling and probably identical with rubiretine, while a brown powder is left undissolved, having the properties and composition of verantine.

0·1755 grm. of the latter, after being purified by redissolving in a boiling mixture of alcohol and ammonia, then adding an excess of acetic acid, collecting the pulverulent deposit formed on cooling, and washing with alcohol, gave 0·4170 carbonic acid and 0·0680 water.

In 100 parts it contained therefore—

|  |  |
|---|---|
| Carbon | 64·80 |
| Hydrogen | 4·30 |
| Oxygen | 30·90 |

The liquid filtered from the lead precipitate is yellow. It gives with water a yellow precipitate, which, after being filtered

* Phil. Mag. vol. v. p. 410.

off and washed with water, dissolves again in boiling alcohol, with the exception of a little brown flocculent matter. The alcoholic solution, on evaporation, leaves a yellow uncrystalline substance resembling impure rubiadine, which contains no chlorine, gives when heated a sublimate like that from rubiadine, and forms with baryta a compound which crystallizes in dark reddish-brown needles. I obtained so small a quantity of this substance that I was unable to arrive at any positive conclusion as to whether it is identical with rubiadine or not. The residue left on evaporation of the alcoholic solution was analysed without any attempt being made to purify it, when it was found to have a composition nearly approaching that of rubiadine.

0·2980 grm. gave 0·7525 carbonic acid and 0·1450 water, corresponding in 100 parts to—

| | | | | | |
|---|---|---|---|---|---|
| Carbon | . | . | . | . | 68·86 |
| Hydrogen | . | . | . | . | 5·40 |
| Oxygen | . | . | . | . | 25·74 |

The formation of this body, as well as that of verantine and rubiretine, from chlororubian, I am unable to explain in a satisfactory manner.

If the excess of sulphuric acid, with which the four last bodies have been precipitated, be neutralized with carbonate of lead, the filtered liquid leaves on evaporation a saline mass, which, on being pulverized and treated with warm alcohol, communicates to the latter a reddish-brown colour. The alcoholic liquid, after being filtered from the insoluble matter consisting of sulphate of soda, leaves on evaporation crystals of chloride of sodium surrounded by a brown syrup of sugar.

*Action of Chlorine on Chlororubian.*—If finely pounded chlororubian be mixed with water, and if, after the mixture has been placed in a large bottle or other suitable vessel, a current of chlorine gas be passed through it, no change is perceptible for some time; but if the bottle be closed, after the space above the liquid has been filled with chlorine, the latter is gradually absorbed, the colour of the chlororubian becomes paler, and after several days it appears perfectly white. The process is not accelerated by the action of sunlight, but frequent agitation of the liquid and powder with the gas assists it. The white powder into which the chlororubian is changed consists of a body to which I will give the name of *Perchlororubian.* After collecting it on a filter and washing out the chlorine and acid with water, it is to be dissolved in boiling alcohol, from which it crystallizes on the solution cooling in colourless, transparent, flat, four-sided tables, exhibiting a beautiful iridescence. If it should not be quite colourless, it must be redissolved in boiling alcohol, to which a

little animal charcoal may be added. On filtering boiling hot and allowing to cool, the solution then yields perfectly colourless crystals. It may also be obtained directly from rubian, by continuing to pass chlorine through a watery solution of the latter, until the yellow precipitate produced at first has become white, but by this means it is not obtained as pure as from crystallized chlororubian.

Perchlororubian has the following properties. When heated on platinum it melts to a brown liquid, and then burns with a smoky yellow flame edged with green, leaving little carbonaceous residue. If slowly and carefully heated it may be entirely volatilized, yielding a sublimate of bright micaceous scales. But if it be suddenly heated, if, for instance, it be thrown into a redhot tube, it is decomposed with a kind of explosion, giving off an acid smell, and forming a large quantity of soot with little or no crystalline sublimate. It is insoluble in water, but soluble in alcohol and æther. The alcoholic solution does not redden litmus paper. Concentrated sulphuric acid dissolves it on heating, the solution, on being heated to the boiling-point, becoming slightly brown, but giving off very little sulphurous acid. The colder parts of the tube become covered with a crystalline sublimate, consisting probably of the substance itself. Nitric acid of sp. gr. 1·37 has no effect on it, even on boiling. Nitric acid of sp. gr. 1·52 dissolves it on boiling without decomposing it, for on adding water, the substance is precipitated unchanged in the shape of a crystalline deposit, and nitrate of silver produces in the liquid no precipitate of chloride of silver. Perchlororubian is quite insoluble in strong caustic soda lye, even on boiling, as well as in ammonia. It dissolves easily, however, in hydrosulphate of ammonia on boiling, and on now adding nitric acid and boiling, nitrate of silver produces an abundant precipitate. The alcoholic solution gives no precipitate with an alcoholic solution of acetate of lead. Its analysis led to the following results :—

I. 0·4945 grm., dried at 100° C. and burnt with chromate of lead, gave 0·6730 carbonic acid and 0·0610 water.

0·4350 grm., burnt with lime, gave 0·7770 chloride of silver.

II. 0·3930 grm. of another preparation gave 0·5330 carbonic acid and 0·0585 water.

0·2730 grm. gave 0·4930 chloride of silver.

These numbers lead to the following composition :—

|          | Eqs. |       | Calculated. | I.     | II.    |
|----------|------|-------|-------------|--------|--------|
| Carbon   | 44   | 264   | 37·09       | 37·11  | 36·98  |
| Hydrogen | 9    | 9     | 1·26        | 1·37   | 1·65   |
| Chlorine | 9    | 318·6 | 44·77       | 44·16  | 44·64  |
| Oxygen   | 15   | 120   | 16·88       | 17·36  | 16·73  |
|          |      | 711·6 | 100·00      | 100·00 | 100·00 |

It appears, therefore, that in passing into perchlororubian, chlororubian loses 9 equivalents of water and 9 of hydrogen, 8 of the latter being substituted by chlorine, since

$$C^{44} H^{27} ClO^{24} + 17Cl = C^{44} H^9 Cl^9 O^{15} + 9HO + 9ClH.$$
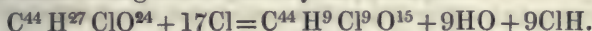
It is a singular circumstance, that the 9 equivalents of chlorine in this compound are much more firmly combined with the other constituents than the 1 equivalent contained in chlororubian, which the mere action of alkali is sufficient to separate.

From the experiments just described it may be inferred, that chlororubian is a conjugate compound containing sugar. It resembles Piria's chlorosalicine, which, by the action of acids, yields chlorosaligenine and sugar, just as chlororubian gives chlororubiadine and sugar. Though chlororubian is not, strictly speaking, a product of substitution of rubian, still it retains some of the properties of the latter; for instance, that of giving, with alkalies, products of decomposition differing from those formed by acids. In all the processes of decomposition previously described, rubian is decomposed in no less than three different modes, just as if it were a compound or mixture of three different bodies, whereas, when acted on by chlorine, it yields only one series of products. It behaves in the latter case as if it were simply a conjugate compound containing sugar. It splits up into sugar and a chlorinated body, and the latter, by the action of acids, again splits up into sugar and a second chlorinated compound. This series of products corresponds exactly with one of the three series in the other processes of decomposition, the bodies belonging to the two other series not making their appearance even in the form of products of substitution.

XXXVI. *A Note on* Professor Stevelly's *Paper on the Theory of Parallels. By* J. P. Hennessy*.

LEGENDRE solves the so-called difficulty of parallel lines by introducing, as it appears to me, the very same principle as that which is employed by Mr. Stevelly. In his *Eléments de Géométrie,* douzième édition, p. 20, he shows that every property of parallel lines can be easily established if the proposition, that the three angles of every triangle are equal to two right angles, be first proved. This proposition, the thirty-second of Euclid, he accordingly makes his first theorem, and he demonstrates it in two different ways. He first gives an elaborate proof of it in the text, and he afterwards deduces it in his notes from the principle of homogeneity. His first and most important demonstration is rigorous

* Communicated by the Author.

and complete. No mathematician can for a moment entertain a doubt as to its correctness. It is, however, open to one fatal objection, an objection which applies with equal force to Mr. Stevelly's demonstration,—that it introduces amongst the propositions of Euclid a method of proof which is totally foreign to the pure and simple reasoning of plane geometry. This is, the doctrine of limits. That Mr. Stevelly employs this method is placed beyond all doubt when he speaks of "making $m'$ approach unlimitedly close to E by taking $b'B = Bb$ unlimitedly small," &c.

But there seems to me to be another objection to Mr. Stevelly's demonstration. The introduction of the method of limits, even if allowable, was quite unnecessary. It is only necessary where we adhere to Euclid's definition of parallel lines. Mr. Stevelly very properly, I think, put aside that definition and substituted one of his own. In the third volume of the present series of the Philosophical Magazine, it will be seen that the question is treated, as far as a change of definition is concerned, in a similar manner. Instead of Euclid's* definition, I made use of the following :—

" Parallel lines are such, that if they meet a third right line, the two interior angles on the same side will be equal to two right angles."

From this, as I stated at the time, all the properties of parallel lines can be deduced. The researches in question were but slightly connected with this subject, and I did not think it necessary to give many reasons for choosing such a definition. I was contented with showing that it obviated in certain cases the necessity of employing *reductio ad absurdum.*

It has, however, always appeared to me that the difficulty which seems to surround the theory of parallel lines is owing, not so much to any inherent peculiarity in the theory itself, as to the manner in which it has been discussed by geometers. There appears to have been an unwillingness to do for parallel lines what has been done for every other geometrical conception. Before investigating the various theorems connected with the square or with the circle, Euclid took a distinct property of each, in logical phraseology a *differentia,* and on that he built his definition. But he did not adopt the same course with respect to parallel lines. In reality he gave no definition whatsoever of them. Attention has been called to the fact that his so-called definition is negative† ; and it is hardly necessary to say, that a negative proposition cannot possibly be considered a definition.

---

* An interesting criticism on Euclid's definition, which supports the view I ventured to take as to its inadequacy, has lately been published. See Wedgwood's 'Geometry,' p. 5.

† Philosophical Magazine, February 1853.

Now when we regard a pair of parallel lines, just as we regard a square or a circle, as a simple geometrical figure, and when we seek for its definition, we find that there are a large number of properties (*propria*) from which to choose.    A different property has been chosen by Wolfius, D'Alembert, Varignon, R. Simson, and a host of others who have written on the subject.    The definition given by Wolfius, but which a distinguished writer* suggests was borrowed by him from Clavius, seems to me, according to analogy, to be the most philosophical.    None of these geometers attempted to solve the difficulty by going back to the fundamental principles of definition, or by carefully analysing those other definitions which have been found to succeed.    It is, however, a remarkable fact, that if we now adopt this course we in one case get a definition exactly the same as that borrowed from the *Geometria Practica.*

Euclid defines a circle to be,—

" A plain figure bounded by one continued line, called its circumference ; and having a certain point within it, from which all right lines drawn to its circumference are equal."

It thus has, like a system of parallel lines, two essential elements. In the one there is a continued line and a point, in the other there are two right lines.    The definition of the circle depends on a property possessed by every possible right line drawn from the point to the continued line.    That property is, mutual equality.    Now we know that the right lines from the point to the continued line are all perpendicular to it.    So that Euclid's definition depends essentially upon the equality of all perpendiculars drawn from one element of the circle to the other.    This leads us very naturally to Wolfius's definition :—

" Parallel right lines are such, that perpendiculars from every point in one upon the other are equal."

From this, as well as from the one I formerly gave, all the properties of parallel lines may be deduced ; the only difference is, that this definition makes the demonstrations rather complex.

If, in the same way, an analysis is made of Euclid's definition of a square, it will be seen to bear a very close analogy to the definition of parallels in which an equality of angles to a constant quantity is made the differentia.    Having defined a rhombus to be " an equilateral quadrilateral figure," he defines a square as " a rhombus each of whose angles is equal to a right angle."

No serious objection has ever been made to this definition ; and yet when we treat parallel lines in a similar way, we are charged with assuming something that ought to have been proved.    I do not think such a charge can be upheld for a single moment. To say, when two parallel lines are intersected by a third

* Dr. Lardner.

right line, that a figure is formed in which the angles at each side are equal to two right angles, is certainly not assuming, either in form or in fact, more than Euclid assumed when defining a square. It has been said that such a definition is imperfect, because we cannot by its aid give a demonstration of the twelfth axiom. This, I think, is another point in which the theory of parallels has been treated unfairly. In the first instance, there is an unwillingness to assume proper premises; on the other hand, when a full and fair definition is chosen, it is said to be inadequate, because it cannot touch certain propositions which are naturally beyond its reach. The twelfth axiom has, in fact, as much connexion with parallel lines, as the theory of the parabola has with that of the circle; yet no one would assert that the definition of a circle was defective because by it we cannot prove, for instance, that "no two right lines in a parabola bisect each other."

Mr. Stevelly appears to think that his demonstration is suited for elementary instruction. I regret that I must also differ with him on this point. As it is merely a matter of opinion, and cannot admit of any proof one way or the other, I do so with extreme diffidence. Perhaps, however, I may venture to remind him, that a distinguished physicist and mathematician like himself is, from the nature of his position, often incapable of forming a very accurate notion as to whether a certain train of reasoning is elementary or otherwise. What may appear very simple to him, may not be so to one whose mental training is of a different character.

Jermyn Street, Sept. 3, 1856.

XXXVII. *On the recent Theories of the Constitution of Jets of Liquid issuing from Circular Orifices. By* M. T. PLATEAU*.

WHEN, in 1849, at the end of the Second Series of my "Experimental and Theoretical Researches on the Figures of Equilibrium," &c.†, I published a complete theory of the constitution of jets issuing from circular orifices, and protected from every external influence, I thought that the same would have been immediately adopted, seeing that it was deduced with absolute rigour from the results of accurate experiments, and consequently that it explained all the details and all the laws of the phænomenon. I was wrong: at the commencement of the

---

* From the *Bulletin de l'Académie de Belgique*, No. 6. vol. xxiii; communicated by the Author.

† *Mém. de l'Acad. de Belgique*, vol. xxiii.   Scientific Memoirs, vol. v. p. 584.

past year two different theories were proposed ; one by Dejean \*, the other by Magnus †. Nevertheless, as I did not find any reference to my theory, and consequently any opposition to it, either in the short analysis which Dejean has given of his still unpublished research, or in Magnus's memoir, I resolved to wait and leave its success to time. Another theory, however, has just appeared. In the report presented to the Academy last January ‡, upon my Third Series §, where I explain the action of vibrations on jets, Maus returns to the constitution of jets not subjected to this action. He declares his inability to adopt my theory, and indicates his reasons for the same; afterwards he developes his own views. As I cannot, under these circumstances, remain longer silent, I propose to compare, as briefly as possible, the three theories above mentioned with my own.

It is well known that Savart, in his able research on jets of liquid ‖, after having by ingenious experiments discovered all the peculiarities in the constitution of jets issuing from circular orifices, gives an essay on the theory of these phænomena in the form of a conjecture simply. He attempts to show that the efflux itself imparts a vibratory motion to the liquid in the vessel, and to that which passes through the orifice; that these vibrations or *pulsations at the orifice,* being perpendicular to the plane of the latter, alternately press out and draw in the liquid as it issues, and thus occasion the annular protuberances or expansions whose existence he established, the movement of translation, and the development of, and final conversion into, isolated masses.

Dejean admits the existence of pulsations, but explains their production differently. In his analysis he does not say to what cause he attributes the separation of the masses which compose the discontinuous part of the jet.

Magnus, in his very interesting memoir, treats principally of the phænomena which manifest themselves when two jets meet under certain angles, and of the singular forms of jets issuing from differently shaped orifices. He enters but little into the question of the constitution of jets issuing from circular orifices. He does not assume the existence of pulsations, and in the case of a jet falling vertically and withdrawn from every external influence, he attributes the separation of the mass to the tension resulting from the acceleration in the velocity of the falling

\* *Comptes Rendus,* vol. xl. p. 467.

† Poggendorff's *Annalen,* vol. xcv. p. 1 ; and Phil. Mag. for Feb. 1856.

‡ *Bull. de l'Acad. de Belgique,* vol. xxiii. part 1. p. 4.

§ We hope to be able to find room for a translation of this Third Series in a future Number of the Magazine.—Eds.

‖ *Annales de Chimie et de Physique,* vol. liii. 1833, p. 337.

liquid; which tension, at a sufficient distance from the orifice, becomes strong enough to overcome the cohesion between two adjoining sections of the jet.

Lastly, Maus adopts, with Dejean, the hypothesis of pulsations, the production of which he, on his part, explains by considerations of his own; but convinced that pulsations alone could not determine the formation of the isolated masses, he assigns the discontinuity of the jet to the same cause as Magnus.

The above theories are based, therefore, upon two hypotheses,— that of pulsations at the orifice, and that of rupture caused by the acceleration in the descent of the liquid. I will now endeavour to show that both these hypotheses are inadmissible.

Before doing so, however, I hope to be allowed to express my deep veneration for the memory of Savart. In my researches on jets I have continually had recourse to the beautiful researches of that illustrious physicist; and although I here combat the hypothesis of pulsations, I still can recognize in the same a trait of genius. In fact, although pulsations do not exist in a jet protected from every external influence, the case is different when vibrations are transmitted to the liquid from an external source. The pulsations then become a reality, they exercise upon the jet the pressures and tractions attributed to them by Savart, and it is by considering these pressures and tractions that I have been enabled to explain, in my recently published Third Series, all the phænomena which depend upon the influence of vibratory motions.

Proceeding to our examination of the two hypotheses in question, let us commence with that of the rupture of the jet. The idea of an effect of this kind produced by acceleration of velocity is anterior to the research of Magnus. Hagen, who made many experiments on jets issuing from circular orifices, speaks of the same in a memoir presented to the Academy of Berlin in 1849, "On the Discs formed by the meeting of two Jets of Liquid, and on the resolution of isolated Jets into Drops\*," but he mentions the idea only in order to refute it. After some *à priori* considerations relative to the probable existence of another cause, he thus expresses himself:—

"This probability becomes a certainty when we reflect that one and the same jet, whether it issues vertically upwards or downwards, resolves itself into drops at almost the same distance from the orifice. In the first case the velocity of the liquid may be almost destroyed by gravity, whilst in the second it may be doubled; and still the phænomenon is not essentially different in the two. The fact is still more striking in horizontal jets, where the effect of gravity almost vanishes. In these the velocity varies only with the increase of inclination; with strong

* Poggendorff's *Annalen*, vol. lxxviii. p. 451.

charges and small orifices, this inclination, and consequently the variations in velocity and diameter, are insignificant in the part of the jet we are considering, so that we should conclude that the jet ought to be perfectly uniform; nevertheless it resolves itself into drops at a determinate point."

To these facts we may add another deduced from Savart's experiments. When a jet issues obliquely upwards, within certain limits of inclination, the discontinuous part becomes scattered in a vertical plane and forms a kind of sheaf. Now a glance at the figure which represents such a jet in the plates accompanying Savart's memoir, will show that the sheaf has its origin very near the summit of the curvature of the jet, and consequently at a point where the velocity, retarded from the orifice, has not yet become sensibly accelerated.

Thus, in the first place, in jets issuing horizontally under a sufficient charge through orifices of small diameter, the discontinuity first occurs at a point where the velocity is scarcely increased, and consequently where the tension between two contiguous sections cannot have become considerable; in the second place, in jets issuing obliquely upwards under a sufficient charge and through a suitable orifice, the discontinuity commences at a point where the increase of velocity is still less; and lastly, in jets issuing vertically upwards, the discontinuity occurs notwithstanding the retardation of velocity; a retardation which, instead of separating two contiguous sections, evidently tends to press them closer towards one another.

Although these reasons ought to be regarded as conclusive, Magnus might nevertheless reply that neither Savart nor Hagen state that the jets in question were protected from the small effects of vibrations proceeding from external noises, and from the shock of the discontinuous part against the liquid into which they fell. Now, according to Magnus, vibrations propagated in a jet are a powerful cause of rupture, because by means of them one section of liquid is drawn backwards, whilst the one immediately preceding it is, on the contrary, pushed forwards. But if so, then in ascending jets the rupture ought to take place at the contracted section itself, that is to say, very near the orifice, for there the jet has the least thickness, and the velocity of translation has not yet suffered sensible diminution. In fact, at a greater distance from the orifice, the increasing thickness of the jet and the gradual diminution of velocity—from which latter results, as I have remarked, a tendency of the sections to approach each other—would render the rupturing action of vibrations more and more difficult.

I have, however, repeated the experiment of the sheaf; first by allowing full liberty to the small effects of external vibrations,

and afterwards by employing the precautions indicated by Savart for deadening these effects. In the first case, the result was sensibly conformable to the engraved figure of which I spoke, which figure no doubt corresponds to conditions of change and orifice analogous to those in my experiment; and in the second case, the result, as will be seen, was not essentially different. The jet issued from an orifice 3 millims. in diameter at an angle of about 35° with the horizon; the amplitude of the jet, that is to say, the horizontal distance from the orifice to the most exterior part of the sheaf, varied, according to the diminution of the charge, from 1·45 metre to 1·27 metre; and the height of the summit of the jet above the level of the orifice varied from 0·34 metre to 0·32 metre. In the second observation, a carpet folded several times until its thickness amounted to 8 centims., was interposed between the vessel from which the jet issued and the massive table upon which it rested, and under the feet of which cushions were placed. The vessel into which the sheaf fell was placed upon a truss of hay, and the sheaf glided down a thick plank suitably inclined. Besides this, the orifice at the bottom was furnished with a contrivance, similar to that described by Magnus, for preventing any rotations in the liquid; the limpid portion of the jet was also regular, or nearly so. Notwithstanding all these precautions, the effect of the small vibrations proceeding from exterior noises was not completely annulled; for the continuous and limpid portion of the jet suffered intermittent shortenings, and even speaking in the neighbourhood of the jet was sufficient to render these shortenings more visible. It will be admitted, however, that the vibrations which still remained must have been very feeble; nevertheless the sheaf manifested itself quite as well as under the free influence of these vibrations, and its origin never receded beyond the summit more than 12 centims.

Again, if in a jet flowing downwards and withdrawn from every external influence, the tension caused by the acceleration of velocity were the cause of disunion, the length of the continuous part ought to be independent of the diameter of the orifice. For under the same charge, the total tension between two contiguous strata of molecules, and the total cohesion which unites the same, will evidently each be proportional to the number of molecules which compose these strata. If, therefore, for any orifice, and at a given distance from the same, the tension were equal to the cohesion, so that the adhesion between two strata was overcome, the same ought to take place at the same distance in a jet issuing under the same charge from a larger or smaller orifice; for at this distance the increase of velocity would be the same as in the first jet, and the total actions of tension and cohesion would have both increased or diminished in the same pro-

portion. On the contrary, however, experiment shows that the length of the continuous portion is nearly proportional to the diameter of the orifice.

Lastly, it is easy to see, *à priori*, that the tension due to acceleration of velocity does not in reality produce any tendency to disunion. In fact, if this tension separated the liquid molecules, the density of the continuous part would diminish from above downwards; now a little reflection on the relative mobility of molecules of liquid will soon convince us that this diminution of density is impossible; for as soon as two molecules tend to separate, another is displaced and steps between them, whence it follows that the only effect of tension will be to lengthen the jet by making it thinner; and as the acceleration is uniform throughout the whole extent of the jet, it is evident that its sole tendency will be to diminish the diameter of the jet from the orifice downwards in a perfectly regular and continuous manner, without occasioning a tendency to rupture at any point.

Thus theory and experiment unite in proving the complete inadmissibility of the hypothesis of rupture. Let us next consider that of pulsations.

We may at once remark, that the latter appears in itself difficult to justify, seeing that Dejean is not satisfied with Savart's explanation, and that Maus attempts another. In fact, in the efflux of a jet completely protected against external influences, where everything appears to be in favour of continuity, the only natural manner of conceiving alternate diminutions and augmentations in the velocity of efflux to arise, would be to suppose, with Savart, that the friction of the liquid against the edges of the orifice sets the latter in vibration, and that these vibrations are transmitted to the liquid. But Savart assured himself that when the borders of the orifice are touched with a solid and resisting body—which ought to arrest, or at least deaden the vibrations,—no change whatever in the nature of the jet is perceptible. The vibrations of the sides of the orifice therefore cannot occasion the pulsations of the liquid; but to whatever cause these pulsations may be attributed, they must, if they exist at all, consist of a vibratory motion of the liquid, which motion would necessarily be communicated to the sides of the orifice; so that by hindering, as Savart did, the vibrations of these sides, the pulsations of the liquid ought to be impeded also, and the state of the jet to be modified; how is it, then, that the jet suffers no appreciable modification?

Further, Savart, when examining the annular expansions of the continuous part of the jet, according to the method he describes, was not able to prove their existence except at a little distance from the orifice; and Hagen, who discovered a method

of his own for this kind of observations, affirms the same thing in his memoir above cited. Hence if these protuberances, which are carried by the translatory motion of the liquid, proceed from the orifice itself, they there project very little. But, according to Savart's experiments, they are more perceptible the further they are observed from the orifice, until near the extremity of the continuous part they become greatly developed, and finally at the extremity they detach themselves successively as isolated masses. Savart does not expressly mention the contractions which alternate with these expansions, but it is evident that the latter cannot become developed and acquire greater volume except at the expense of the intermediate portions, so that these parts must necessarily become hollower and thinner the more the protuberances project. Now let us suppose for a moment that the expansions and contractions are due to the pulsations at the orifice, and that, as Savart thought, this vibratory motion communicates itself to all the continuous part of the jet. According to this hypothesis, each portion of the liquid pressed outwards, when leaving the orifice, by a compressing pulsation, would during its transport soon become changed into a contraction by the opposite vibration which it would execute, then again into an expansion, afterwards into a contraction, and so on; whilst each portion originally contracted by a receding pulsation would at the same time suffer inverse variations. But, according to the above, these successive changes of each portion of liquid, or the vibrations they perform during their movement of translation, increase in amplitude from the orifice to the extremity of the continuous part, and that in an enormous proportion; for near the orifice the expansions and contractions are so minute, that by the most delicate method of observation their commencement cannot be ascertained. Now it is impossible to account for this increase in the amplitude of the vibrations in the jet, which increase, in fact, would constitute an effect without a cause.

From this examination, we conclude that the hypothesis of pulsations at the orifice, engendered by the efflux itself, and causing a vibratory motion in the jet, ought to be rejected, as well as that of rupture produced by the acceleration in the velocity of translation of the liquid.

In order to omit nothing, I ought to mention that Magnus denies the existence of these expansions in a jet protected from every external influence. But his only motive for doing so is the aspect of the limpid portion of the jet, which appears perfectly white. " It has the appearance," says he, " of a solid of revolution formed of the whitest glass, for the least motion cannot be perceived in it." Now Savart, when speaking of the limpid portion of jets freely exposed to the small vibratory motions

proceeding from external noises and the fall of the liquid into the vessel which receives it, expresses himself thus :—" No traces of protuberances can be detected in this portion of the jet;" and in the *résumé* at the end of the memoir, he says, " This first part of the jet is calm and transparent, and resembles a rod of crystal." Nevertheless it is in this same portion of the jet that he has proved, by means of his ingenious contrivance, the presence of expansions the more developed the greater the distance from the orifice at which they are observed. The non-existence of expansions, therefore, cannot be concluded from the quiescent and white appearance of the limpid portion of a jet : this appearance is merely an illusion caused by the small prominency of the expansions in this portion of the jet, and the rapidity of their translatory motion. Had Magnus employed Savart's method, or that of Matteucci*, or one of those of Billet-Sélis†, he would certainly have detected the existence of expansions, even when the jet was protected from external vibratory action; for seeing that rupture is impossible, the separation of the mass must necessarily be ascribed to the formation of contractions which become more developed as they approach the extremity of the continuous part, and which necessitate expansions whose development passes through the same phases of augmentation.

By the side of the inherent difficulties of the hypotheses just discussed, let us place the principles of my theory.

All physicists are now acquainted with my method of neutralizing the action of gravity upon a large mass of liquid, and at the same time leaving it free to obey molecular actions. By means of this, and afterwards of another method, I have been able to obtain liquid cylinders and to study their properties. I have thus corroborated the following facts (see my Second Series, §§ 37 to 68)‡ :—

1. A liquid cylinder constitutes a figure of stable equilibrium as long as the ratio between its length and its diameter does not exceed a certain limit between 3 and 3·6. I will here add, that an *à priori* method, of which I have hitherto only published the result, gave for exact value of the limit the quantity $\pi$, that is to say, the ratio between the circumference and the diameter of a circle.

2. Beyond this limit the cylinder constitutes a figure of unstable equilibrium, so that it cannot be obtained in a permanent state except by means of certain hindrances.

3. A liquid cylinder whose length is very great in comparison to its diameter, converts itself, by the spontaneous rupture of

* *Comptes Rendus,* 1846, vol. xxii. p. 260.
† *Annales de Chimie et de Physique,* 1851, vol. xxxi. p. 326.
‡ Taylor's Scientific Memoirs, vol. v. p. 621.

equilibrium, into a series of isolated spheres, equal in diameter, equidistant, and having their centres on the line which forms the axis of the cylinder; in the intervals between these are spherules of different diameters, having their centres on the same line.

4. This transformation commences with the origination of regularly placed contractions alternating with expansions; afterwards both one and the other become more developed; the contracted parts becoming thinner, the expanded ones thicker. When the centres of the contracted parts become sufficiently thin, they do not rupture suddenly, but the liquid, receding on both sides of each of the centres towards the expanded parts, still leaves the latter for an instant connected two and two by a thread sensibly cylindrical. Lastly, these threads transform themselves in the same manner as the cylinders; and by the rupture of threads still more attenuated, resulting from their own contractions, they leave the isolated masses which form the above spherules, whilst the larger masses proceeding from the expanded parts of the original cylinder, and which are at equal distances asunder, assume spherical figures.

5. This spontaneous alteration and this transformation, whose final result is the formation of isolated spheres with spherules arranged in the intervals, are not peculiar to cylinders : they accompany every other liquid figure of which one dimension is considerable compared to the other two.

Lastly, I recognized many other properties which will be found described in my Second Series, but which need not be here repeated.

For the sake of those who have not repeated my experiments, I will here mention, in support of the above facts, a phænomenon observed by all physicists. When, along a thin wire held horizontally, an electric discharge capable of fusing, but not of melting it, is passed, the wire becomes first heated to a white heat, and at the same time bent in consequence of its elongation; afterwards it is observed to resolve itself into a great number of separate globules, which fall, and whose form when cool is found to be rounded. Now this wire, at the moment of fusion by the passage of electricity, constitutes a liquid figure which satisfies the condition above expressed in 5.

I may add that Beer, in a memoir published in 1855*, after having applied mathematical analysis to the facts described in my first series, incidentally treats of the question of the stability of liquid cylinders. Starting solely from data relative to the molecular constitution of liquids, he arrives, by *à priori* reasoning and by calculation alone, at the result which I have myself

* Poggendorff's *Annalen*, vol. xcvi. pp. 1 and 210.

obtained, though by an entirely different method; that is to say, he finds for the exact value of the limit of this stability the quantity $\pi$. This calculation, therefore, which fully corroborates mine, sanctions one of the principal results of my observations; it shows the absolute necessity of it, and gives the precise measure of the phænomenon.

Now a jet of liquid issuing in any direction whatever, fulfils the condition expressed in 5; that is to say, it constitutes a liquid figure whose length is considerable when compared with its transversal dimensions. It should then of necessity alter its form, in order to transform itself gradually and in a continuous manner into a series of isolated spheres with interposed spherules; and the phænomenon ought to manifest itself by the formation of contractions and expansions which develope themselves more and more without changing their nature, until the generation and rupture of the threads take place as above described. But the liquid of the jet having a motion of translation, which carries with it the expansions and contractions, it is during this journey that each of them completes all its progressive modifications; so that each protuberance leaving the orifice in its rudimentary state, does not attain to that of an isolated mass until at a greater or less distance from the orifice. Further, a fresh quantity of liquid being always supplied, the transformation must be incessantly repeated. Hence the continuous and discontinuous parts of the jet; hence also the origination of expansions and contractions scarcely perceptible near the orifice, but becoming more and more developed as they move onwards with the liquid, until the expansions arriving one after another at the extremity of the continuous part, successively detach themselves and pursue their course as isolated masses, which assume, or tend to assume, a spherical form; hence, too, the spherules interposed between these masses; and lastly, the laws discovered by Savart, connecting the length of the continuous part, as well as the tone produced by the shock of the jet, with the charge and the diameter of the orifice. To render this last deduction evident, however, it would be necessary to have recourse to facts, which, fearing to extend this note too much, I have not here stated.

It will be seen that the fundamental part of my theory consists, not of hypotheses, but of facts and their inevitable consequences; which consequences are so strict, that, had not the admirable research of Savart revealed to us the true constitution of jets of liquid issuing from circular orifices, we might have predicted the same with all its details and laws from my experiments. My theory does more: it shows that the laws indicated by Savart are only the limits, which are more and more approached the stronger the charges and the smaller the orifices, a deduction

which the tabulated observations of the illustrious French philosopher fully confirm.

I have still to establish the connexion between my theory and the phænomena resulting from the influence of tones ; as already stated, this was done in my Third Series. When vibrations are in reality communicated to the vessel and the liquid it contains, the principle of pulsations at the orifice ceases to be a vague and improbable hypothesis, and becomes almost a certainty. Now I have demonstrated that when the tone which acts upon the jet is exactly in unison with the one which the shock of the discontinuous part against a stretched membrane would produce, the expansions and contractions caused by the vibrations executed at the orifice coincide perfectly with the expansions and contractions due to the molecular forces, so that there is absolute concurrence between the two actions. From this simple principle I have afterwards deduced the explanation of all the phænomena observed by Savart.

If, in this collection of theoretical researches, I have had recourse to hypotheses, they are purely accessory, and serve only to explain certain facts too complicated to admit of our appreciating the action of the several elements concerned in their production.

Let us pass, lastly, to the arguments upon which Maus bases his opposition. They are stated in the following passage of the report :—

. . . . . . " I find it difficult to admit, with Plateau, that in a phænomenon principally produced by gravity, this force ought to be completely eliminated in order to attribute the configuration of the moving mass exclusively to molecular forces, which are far inferior to gravity.

" My hesitation increased when I found, that, in order to justify the elimination of gravity, Plateau considered this force as only acting upon the jet when leaving the orifice, without considering its action upon the liquid contained in the vessel ; which action, by its manner of attracting the liquid molecules towards the orifice, exercises upon the form of the jet an influence which cannot be doubted when we consider the phænomena known as the *contraction* and *inversion* of a jet.

" The beautiful experiments with jets of liquid, repeated every year by Prof. Richelmi at the hydraulic establishment of La Parella near Turin, having shown me a kind of opposition between orifices of different shapes and the profiles which they produce in the jet at a short distance from them, have not prepared me for disregarding the interior action on the form of the jet."

It is impossible to find real objections here. Without doubt gravity causes the liquid to leave the vessel and thus produce

the jet; but once having left the orifice and acquired its translatory motion, the liquid is left to the free action of its molecular forces, whether the same be more or less energetic than gravity; just as the hands of a watch continue to be moved by internal mechanism, whilst it itself is transported from one place to another. Maus, indeed, himself admits this when, in another place, speaking of the acceleration of velocity as a cause of rupture, he says, "and when this action has been prolonged sufficiently to divide the liquid mass, molecular forces intervene in order to give to the isolated masses the spherical form which the drops possess."

The contraction of the jet plays an important part in the theory of Maus; but in mine, the simple consequence of this phænomenon of contraction is, that I have not to consider the action of molecular forces except from the transverse section of the jet where this contraction ends, for it is only beyond this point that the jet assumes its elongated form.

With respect to the inversions which manifest themselves in jets issuing from non-circular orifices, I may remark that they have nothing in common with the expansions and contractions which precede the separation into isolated masses; for the latter are carried with the translation of the liquid, whereas the inversions occupy fixed positions in the limpid portion of the jet. Further, Maus will find, on referring to that part of Magnus's memoir which treats of the influence of orifices of different shapes, that it is precisely by causing the intervention of molecular forces that the author has completely succeeded in explaining these inversions.

I cannot conclude without acknowledging the merit of Magnus's research as a whole. As already remarked, the passages to which I have objected form but a small part of the whole; the rest appears to me very remarkable, as well in a theoretic as in an experimental point of view.

XXXVIII. *Chemical Notices from Foreign Journals.*
*By* E. Atkinson, *Ph.D.*

[Continued from p. 196.]

HEINTZ* communicates the particulars of some experiments with chloroform. They mostly afforded negative results, but possess nevertheless a considerable degree of interest.

In the expectation of producing formyle, $C^2H$, he enclosed sodium along with chloroform in a sealed tube and exposed it to a high temperature. But no action was observed, although the

* Poggendorff's *Annalen,* June.

temperature was kept for some time at 200°C. When potassium is heated in chloroform vapour a violent explosion takes place.

By acting on formiate of lead with chloroform, he hoped to get anhydrous formic acid, thus :—

$$3(PbO \ C^2HO^3) \ + \ C^2 \ H \ Cl^3 \ = \ 3PbCl + 4(C^2HO^3).$$
Formiate of lead.    Chloroform.        Anhydrous
                                        formic acid.
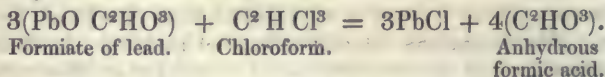
But formiate of lead is not acted upon by chloroform at any temperature at which it would not be decomposed if heated alone. For he found, on heating formiate of lead to about 200° C. under exclusion of air, that it is decomposed into metallic lead, carbonic acid, and hydrogen.

Heintz thought that by acting on chloroform with ammonia, if any action took place which was not accompanied by decomposition of the formyle, it might be according to two equations :—

$$C^2 \ H \ Cl^3 + 4NH^3 = 3(NH^4 \ Cl) + C^2 \ H \ N,$$
Chloroform.

$$C^2 \ H \ Cl^3 + 6NH^3 = 3NH^4 \ Cl + 3(NH^2)C^2H.$$
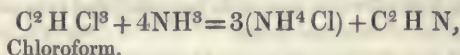
By the first of these actions a nitride of formyle might be obtained, and by the second a formyltriamine.

At ordinary temperatures the two substances do not act on each other. When chloroform is passed with excess of ammonia through a heated tube, no action takes place till the temperature approaches a red heat, when chloride and cyanide of ammonium are formed :—

$$C^2 \ H \ Cl^3 + 5NH^3 = 3(NH^4 \ Cl) + NH^4C^2N \ ;$$
Chloroform.            Chloride of    Cyanide of
                       ammonium.      ammonium.
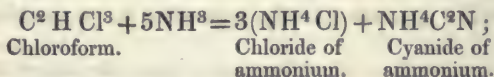
and if the temperature be raised high enough, paracyanogen is produced from the decomposition of the cyanide.

The action is different in the presence of water. In that case it may be expressed by the formula—

$$C^2 \ H \ Cl^3 + 4HO + NH^3 = NH^4 \ O, \ C^2HO^3 + 3(NH^4 \ Cl).$$
Formiate of ammonia.
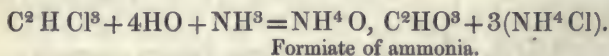
If the aqueous solution of ammonia with chloroform be heated to 180° C., formiate of ammonia and chloride of ammonium are formed. If the alcoholic solution of ammonia be heated with chloroform to 190°, some formiate of ammonia is formed, together with cyanide of ammonium. Sometimes neither the one nor the other of these bodies is produced; when this is the case, paracyanogen is formed in some quantity. Along with these bodies a little æthylamine is formed; but this arises from the action of ammonia on the alcohol, and is quite independent of the chloroform.

The same chemist investigated * the action of chloride of sulphur on formiate of baryta and acetate and benzoate of soda. By the first of these actions he hoped to get anhydrous formic acid. But such is not the product of the action; there are formed monohydrated formic acid and carbonic oxide: thus—

$$4(BaO, C^2HO^3) + 3ClS = 2S, BaO\,SO^3, 3BaCl, 4CO, 2(C^2HO^3HO).$$

Formiate of                                       Monohydrated
    baryta.                                          formic acid.
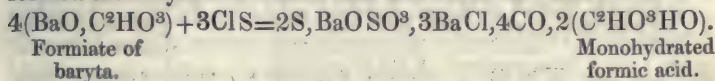
The reaction gives a method for the preparation of monohydrated formic acid, which consists in mixing four equivalents of an anhydrous formiate with four equivalents of water, and then adding three equivalents of chloride of sulphur. By distilling the mixture at a temperature of 100° C. to 120° C., pure monohydrated formic acid is obtained.

An acid formiate of soda or potash cannot be formed, and hence the method used by Melsens for producing monohydrated acetic acid from binacetate of potash is inapplicable in the case of formic acid.

Heintz found that chloride of sulphur acts on anhydrous organic salts in such a manner, that a chloride and sulphate of the metal are produced and sulphur separated; and the sulphur separated crystallizes from boiling alcohol partly in the form of rhombic octahedra, and partly in that of long acicular crystals. The same is the case with sulphur separated by acids from solution of sulphide of sodium or potassium.

Anhydrous acetate of soda gives with chloride of sulphur anhydrous acetic acid according to the equation—

$$4(NaO, C^4 H^3 O^3) + 3Cl\,S = Na\,Cl, NaO\,SO^3, 2S, 4(C^4 H^3 O^3).$$

Acetate of soda.                                      Anhydrous acetic
                                                     acid.
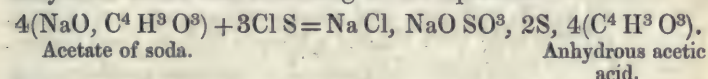
But there are formed at the same time various collateral products, especially sulphur compounds, and in certain cases a liquid heavier and less volatile than water, and not miscible with it.

In the endeavour to produce anhydrous acetic acid by heating acetate of mercury, there were produced acetone, and a substance containing more oxygen than it, but which was not formic acid.

When chloride of sulphur acts on anhydrous benzoate of soda, chloride of benzoyle is first formed, which reacting on an excess of benzoate of soda, produces anhydrous benzoic acid and chloride of sodium :—

$$2(C^{14}H^5O^3, NaO) + 3ClS = 2S + NaO\,SO^3, + NaCl + 2(C^{14}H^5{}^{O^2}_{Cl}),$$

Benzoate of soda.                                            Chloride of
                                                     benzoyle.

$$C^{14} H^5 {}^{O^2}_{Cl} + (NaO, C^{14} H^5 O^3) = 2(C^{14} H^5 O^3) + Na\,Cl.$$

Chloride of      Benzoate of soda.      Anhydrous
benzoyle.                                 benzoic acid.
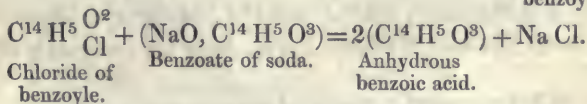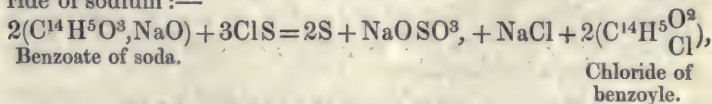
* Poggendorff's *Annalen*, July.

This method of preparing anhydrous benzoic acid has the advantage over the original method of Gerhardt, that the material chloride of sulphur is cheaper and more easily prepared than the pentachloride and oxychloride of phosphorus.

Heinrich Rose* obtained boracic æther by distilling together a mixture of sulphovinate of potash with excess of fused boracic acid. At a temperature of 100° to 120° C., a colourless mobile liquid, of a strong ætherial odour, passes over, having all the properties ascribed to boracic æther by its discoverer. If, in the preparation of this body, anhydrous materials be not employed, water passes over with the distillate and gradually decomposes the æther, boracic acid being liberated.

The analysis of the æther was made in the following manner:— A weighed quantity, 0·967 grm., of the æther was left in a closed vessel in contact with water for many days. It was then evaporated to dryness with a weighed quantity, 2·180 grms., of carbonate of soda; the mass gently ignited and weighed. It weighed 2·260 grms. The carbonic acid contained in this was determined; it amounted to 0·736 grm., and hence the boracic acid was 0·246 grm., or 25·44 per cent., the theoretical number being 24 per cent. Ebelmen, the discoverer, found 22·3 per cent., but the method of analysis employed by him involved a loss.

Rose attempted by the same method to produce from silicate of soda, and carbonates and phosphates of the alkalies, the corresponding silicic, carbonic, and phosphoric æthers, but in vain. Equally unsuccessful were similar attempts to form from bichromate and from molybdate of potash their respective æthers.

No aluminic æther was obtained by distilling pure aluminate of soda (prepared by heating together atomic weights of alumina and carbonate of soda) with sulphovinate of potash. Nor by the action of chloride of aluminium on alcohol was a more favourable result arrived at.

Wilkens† has an article on artificial ultramarine, in which he adverts to the views put forth by Stölzel‡ on the constitution of this body. Wilkens considers that in artificial ultramarine a certain constant compound is contained, but mixed with other foreign bodies. Resting on a number of careful analyses, as well as on its reactions and properties, he considers blue ultramarine to have the formula—

$$2(Al^2O^3 \, SiO^3) + Al^2O^3 \, 3SiO^3 + NaO \, S^2O^2 + 3NaS.$$
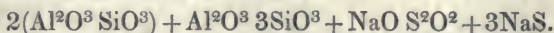
The formula which Wilkens derives from his analyses is different

* Poggendorff's *Annalen*, June.
† Liebig's *Annalen*, July. ‡ Phil. Mag. June.

from that proposed by Breunlin*, who considers it to be a compound of a mineral of the formula of nepheline with pentasulphide of sodium.

But both chemists agree in the fact, that when decomposed by acids, one equivalent of sulphur is liberated as sulphuretted hydrogen, and four equivs. as free sulphur. Wilkens thinks that hyposulphite of soda, with sulphide of sodium, is the colouring principle of a silicate of alumina. The sulphur in ultramarine cannot be replaced by any other body, nor can the sodium. Experiments carefully made with this view, with potash, lime, and magnesia, afforded no ultramarine. Iron exerts no influence in producing the blue colour.

Green ultramarine, which always precedes the formation of blue, is considered by Wilkens to be a sulphide of sodium compound, but he had arrived at no certain results as to its constitution, since, from the various methods of its formation, it occurs of very different form and composition.

Dr. Grimm† describes a new platinum salt. On adding caustic ammonia to a hydrochloric acid solution of protochloride of platinum, which had not been neutralized with carbonate of ammonia, a beautiful garnet-red crystalline compound was thrown down. On continuing the addition of ammonia, Magnus's salt gradually formed in green flakes; but the same red crystals were constantly formed, and only disappeared on long-continued boiling with excess of ammonia. They were once obtained in distinct quadratic octahedra of a ruby colour.

The salt is difficultly soluble in cold, but more easily in hot water. It is insoluble in alcohol, but completely soluble in ammonia and carbonate of ammonia.

The analyses lead to the supposition that it is a compound of protochloride of platammonium with chloride of ammonium, $\left. {Pt \atop H^3} \right\} N, Cl + NH^4 Cl$; but as some of its reactions are not in accordance with this view, the author reserves a decision on the point for further investigation.

Voit‡ investigated and describes several benzoyle compounds, derivatives of benzoic acid. In the preparation of nitrobenzoic acid a slight modification of the usual method was adopted. Melted benzoic acid was added in small portions to a mixture of strong sulphuric and nitric acids and gently warmed for half an hour. By this method no binitrobenzoic acid is formed; and on

* Phil. Mag. June, Supplement.
† Liebig's *Annalen,* July, p. 95.     ‡ Ibid. p. 100.

the addition of water to the acid mixture, nitrobenzoic acid alone is precipitated.    The acid is also obtained by adding chloride of benzoyle to the acid mixture.

The nitrobenzoic acid was converted into benzamic acid, for which Voit proposes the name *amidobenzoic acid,* as expressing its constitution better.    This was effected in the usual manner by treatment with sulphide of ammonium.    After filtering off from the separated sulphur and adding hydrochloric acid, a compound of amidobenzoic acid with that acid is obtained.

Of amidobenzoic acid, the silver salt alone had been hitherto known.    Gerland studied some compounds which it forms with acids, in which it plays the part of a base.    Voit describes in this paper the baryta, lime, soda, magnesia, and strontia salts, all of which are crystallizable, and perfectly well defined.    The lime and baryta salts were prepared by neutralising the hydrochloric acid compound of amidobenzoic acid with their carbonates.    From the baryta salt the others were prepared.

Binitrobenzoic acid is obtained by boiling benzoic acid with a mixture of sulphuric and nitric acids until the evolution of nitrous fumes ceases.    On allowing the mixture to cool, the acid is deposited in brilliant crystals.    It is more difficultly soluble in water than nitrobenzoic acid.

By treating this acid with sulphuric acid and alcohol, oily drops are formed, which on standing become solid.    These drops are binitrobenzoate of æthyle.    It is soluble in warm alcohol, from which it crystallizes on cooling in long silky needles.    By treating this æther with an alcoholic solution of ammonia, the *binitrobenzamide* is obtained, crystallizing in silvery scales.    By acting on an ammoniacal solution of binitrobenzoic acid with sulphuretted hydrogen, it is converted into *binamidolenzoic* acid, sulphur being eliminated.    On the addition of hydrochloric acid to the solution filtered off from the sulphur, green crystals of a compound of hydrochloric acid and binamidobenzoic acid are gradually formed.    Binamidobenzoic acid is obtained in the free state by digesting its compound with sulphuric acid with carbonate of baryta.    It crystallizes in small greenish-pointed crystals, which are soluble in water, alcohol, and æther, tasteless, and without action on vegetable colours.

Binamidobenzoic acid is remarkable from the property of combining with acids, which it possesses in a high degree.    It exhibits a similar comportment to that observed in the case of aniline,—that substitution products retain in general the chief character of the compound from which they are derived, but that the replacing substance does not entirely lose its chemical character in the substitution product, but impresses it more upon the compound as the number of equivalents replaced by it in-

creases. Benzoic acid is known as a strong acid; with the entrance of one equivalent of amide, which induces basicity, basic properties show themselves, for amidobenzoic acid forms compounds not only with bases, but with acids; with the entrance of a second equivalent of amide, the properties of an acid are entirely lost, for binamidobenzoic acid forms compounds with acids alone.

The formula of the hydrochloric acid compound of binamidobenzoic acid is $C^{14} H^4 (NH^2)^2 O^4, 2HCl$. It crystallizes in white needles; it is precipitated by strong hydrochloric acid from its aqueous solution in flocks, which change into these needles. When its aqueous solution is mixed with bichloride of platinum, a body is obtained which appeared to be

$$C^{14} H^4 (NH^2)^2 O^4, 2(HCl, Pt Cl^2).$$
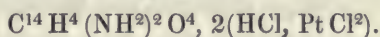
The sulphuric acid compound of binamidobenzoic acid is obtained by dissolving the hydrochloric acid compound in dilute sulphuric acid. Its formula is $C^{14} H^4 (NH^2)^2 O^4, 2SO^3 HO$. It crystallizes in colourless plates. With nitric, acetic, and oxalic acid, binamidobenzoic acid forms well-defined compounds, all of which crystallize.

Limpricht[*] found that the action of acetate of iron on nitrobenzoic acid was the same as that of sulphide of ammonium, benzamic acid being produced.

In the hope of obtaining a compound corresponding to the alcohol radicals, Limpricht[†] tried the action of chloride of benzoyle on aldehyde-ammonia. But instead of the body $\left. \begin{array}{l} C^{14} H^5 O^2 \\ C^4 \ H^3 O^2 \end{array} \right\}$, which would be formed according to the following equation,—

$$\left. \begin{array}{l} C^{14} H^5 O^2 \\ Cl \end{array} \right\} + \left. \begin{array}{l} C^4 H^3 O^2 \\ NH^4 \end{array} \right\} = \left. \begin{array}{l} C^4 \ H^3 O^2 \\ C^{14} H^5 O^2 \end{array} \right\} + NH^4Cl,$$
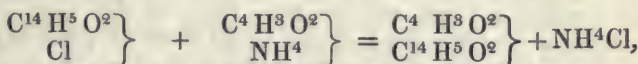
Chloride of benzoyle.    Aldehyde-ammonia.    New body.

a substance was obtained having the composition $C^{32} H^{16} N^2 O^4$, of the production of which Limpricht gives no explanation. It crystallizes from alcohol in fine concentrically grouped needles. It is not soluble in water. Treated with potash, it gives ammonia, resin of aldehyde, and benzoic acid. With peroxide of lead and sulphuric acid, benzamide and aldehyde are formed. These bodies are also the product of the action on it of nitrous acid. Its alcoholic solution reduces neither silver nor mercury. Limpricht observes that the formula contains the elements of benzamide

* Liebig's *Annalen,* July, p. 118.        † Ibid. p. 119.

and cinnamide,

$$C^{18} H^9 NO^2 + C^{14} H^7 N O^2 = C^{32} H^{16} N^2 O^4,$$
Cinnamide.     Benzamide.
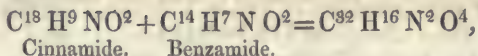
and also that the analyses of hipparaffine (a product of decomposition of hippuric acid) agree very closely with those of this body.

Dr. Vohl* describes a triple salt of sulphate of magnesia, manganese, and zinc. It is obtained by leaving to spontaneous evaporation, at a temperature of 9° C., a mixture in equivalent proportions of the sulphates of magnesia, zinc, and manganese. It crystallizes in large plates, and columns of a clear rose colour of the form of sulphate of magnesia, and has the following composition :—

$$MgO, SO^3 + MnO, SO^3 + ZnO, SO^3 + 21HO.$$
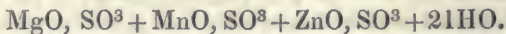
If sulphate of potash or ammonia be added, the corresponding triple salt, with 18 equivs. of water, is obtained, 3 equivs. of water being replaced by an alkaline sulphate.

The same chemist obtained† from the unripe fruit of the *Phaseolus vulgaris,* a new saccharine matter. The fresh unripe fruit is heated some time in steam, or dipped in boiling water and then pressed in a mill. The juice is mixed with yeast and submitted to fermentation, then neutralized with chalk, the mass evaporated and exhausted with alcohol. On gentle evaporation, a body crystallizing in needles was obtained. The comportment of this substance with sulphate of copper and free alkali, as well as its unfermentiscibility, place it near mannite; on the other hand, its comportment in dry air and its composition, are very different from those of that body. The formula of the body dried at 100° C. is $C^{21} H^{21} O^{20}$, that of mannite is $C^{12} H^{14} O^{12}$. Vohl proposes for it the name *Phaseomannite,* and promises a more complete account when he is able to prepare more substance.

Gössmann‡ recommends the use of manganate of potash as a decolorizer of organic bodies, and states that he has employed it in purifying uric, hippuric, and cyanuric acids, with great success.

* Liebig's *Annalen,* July, p. 124.     † Ibid. p. 125.
‡ Ibid. Sept. p. 373.

## XXXIX. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 233.]

February 7, 1856.—Colonel Sabine, R.A., V.P. and Treasurer, in the Chair.

THE following communications were read :—

"On the Vitality of the Ova of the Salmonidæ of different Ages; in a Letter addressed to Charles Darwin, Esq., M.A., V.P.R.S. &c." By John Davy, M.D., F.R.S. Lond. and Edinb. &c.

My DEAR SIR,—In a letter which I had the honour to address to you last year "On the Ova of the Salmon in relation to the distribution of Species," I have expressed the hope that some of the results of observations therein described may aid in solving the question as to the period, the age, at which the impregnated ova of fish are most retentive of life, and consequently are in the state best fitted for transport without loss of life.

Joining with you in considering the subject in need of and deserving further inquiry, I have taken the earliest opportunity that has offered of resuming it. The experiments which I have made, and which I shall now describe, have been more limited than I could have wished, having been confined to the ova of the Charr, as I was not able to obtain the ova of the Salmon or any of its congeners in a fit state for the trials required.

The ova of the Charr which have been the subject of my experiments, were from living fish brought to me from the river Brathay, a tributary of Windermere, on the 9th of November. They were obtained by the pressure of the hand on the abdomen of the females under water, and immediately after their expulsion a portion of liquid milt, procured in the same way from a male, was mixed with them for the purpose of impregnation.

The ova thus treated, 654 in number, procured from two fish, were transferred, after little more than an hour, to a shallow glazed earthenware pan, of a circular form, about a foot in diameter, without gravel, the water in which, afterwards, was changed daily once, and once only. The vessel was kept in a room of a temperature fluctuating from about 55° Fahr. when highest, to about 40° when lowest. The water used was well-water of considerable purity, and before used it was allowed to acquire the temperature of the room.

Two modes occurred to me as likely to afford the means of testing the vital power of the ova, or their power of endurance without loss of vitality; viz. one by subjecting them for a limited time to a temperature raised above the ordinary temperature; the other, by having them conveyed to a considerable distance.

For the trials first proposed, the ova were put into a thin glass vessel half-full of water, which was placed in a water-bath and heated to the temperature desired.

The first experiment was made on ova taken from the general stock one day after their expulsion. Six, for two hours, were exposed to a temperature varying from 79° to 80° of Fahr. The result was,

that they became opaque in the course of twenty-four hours, all but one, and that, some days after, underwent the same change, denoting loss of vitality.

The second experiment was made on the 10th of November. Six ova were similarly exposed for two hours to a temperature rising gradually from 70° to 78°; the result was similar: on the following day they were all found opaque.

The third experiment was made on the 11th of November. The same number of eggs were exposed for an hour to a temperature falling from 70° to 69°. Two shortly became opaque; four retained their transparency during a month, though in reality dead, which was denoted by their bearing no marks of development when seen under the microscope, those ova which retained their vitality being at that time well advanced.

The fourth experiment was made on the 1st of December; the ova, the same number, were exposed to a temperature rising from 75° to 78° for an hour and twenty-two minutes. Three became opaque, other three retained their transparency and vitality, and in due time were hatched, the first on the 31st of December, the last on the 7th of January.

The fifth experiment was made on the 13th of December. Six ova were exposed for an hour and twenty-five minutes to a temperature falling from 82°, which it was at the beginning, to 78°, which it was at the end. Two became opaque; in these no marks of progress could be seen of development, thus indicating that they were dead at the time of trial. Four remained transparent; in these, under the microscope, embryo-fish were seen with an active circulation of the blood-corpuscles. One of them was hatched on the 31st of December; one, the last, on the 6th of January.

The sixth experiment was made on the 20th of December, on six ova, containing living embryos. They were exposed for an hour and twenty-eight minutes to a temperature of about 98°, and this during the whole time. When taken out, they had not lost their transparency, but in each the heart's action was arrested, and death was the result: they all sooner or later became opaque, from the common cause, the imbibition of water.

The seventh experiment was made on the 21st of December, on six ova, in which the circulation was distinct in the fœtal fish. After an exposure for an hour and five minutes to a temperature of 70° rising to 82°, in five, on cooling, the circulation was found active; in one, stopped, which was dead; two were hatched on the 5th of January; three, the remainder, on the 7th of the same month.

The eighth experiment was made on the 23rd of December, on six ova, each containing a living fœtus. They were exposed to a temperature falling from 84° to 82° during an hour and twenty minutes. Examined after the water had cooled, in one, the circulation was seen pretty distinct; in two, very feeble; in three, the blood-corpuscles appeared to be stagnant. Examined on the following day, the circulation was seen active in all. One was hatched on the 5th of January, the other five in the two following days.

The ninth experiment was made on the 24th of December. Six ova were exposed for two hours and four minutes to a temperature falling from 72° to 70°. Examined a quarter of an hour after, and before the water was cold, the circulation was found vigorous in all. One was hatched on the 2nd of January, the remainder between the 5th and 8th.

The tenth experiment, and the last of its kind that I have to describe, was made on the 2nd of January. Six ova, in each of which the circulation was distinct, were exposed for four hours to a temperature varying from 70° to 72°—the greater part of the time 72°. Examined immediately on being taken out, the circulation was seen uninterrupted in three, arrested in the other three. In three-quarters of an hour, when the water had cooled nearly to the temperature of the room, 55°, the circulation was found to be renewed in the latter. In the interval, one of the former was hatched, and a vigorous fish produced; on the following morning four more had come forth, and in the one remaining egg the fœtal circulation was vigorous; it was hatched on the 4th of January.

I beg now to pass to the other series of experiments referred to, those in which trial of the vitality of the ova was made by sending them to a distance. The method was briefly the following. The ova were lightly packed in wet wool contained in a tin-plate box perforated in its bottom to admit air, and covered with a wooden cover that had been soaked in water, with the intent of preserving moisture. The box was wrapped in tow, loosely covered with oiled paper, and the whole, in an envelope of common writing-paper, was well secured by a binding of thread. Thus prepared, the ova were sent by post to Penzance, in Cornwall, a distance exceeding 500 miles, with the request that they should be sent back by return of post unopened.

The first experiment was made on the 9th of November. The number of ova sent was thirty, taken from the common stock without selection. They were received on their return on the 14th of the same month. On taking them out, all were found transparent; but, with the exception of one, all became opaque on being put into water, and that one, after a few days, also underwent the same change.

The second experiment was made on the 14th of November. Twenty ova then sent were returned on the 18th. All became opaque on being put into water.

The third experiment was made on the 1st of December. Twenty ova then sent were returned on the 5th. Put into water, eleven became opaque within a minute; most of these were slightly shrivelled. After three hours, two more became opaque. After forty-eight hours, four only remained transparent; in these, under the microscope, the circulation was found active in two; in the other two it could not be detected. One was hatched on the 31st of December, the other died before hatching.

The fourth experiment was made on the 13th of December. Twenty-two ova then sent came back on the 17th. During the interval there was a severe frost; the thermometer here in the open air was constantly below the freezing-point, and it would appear to

X 2

have been much the same throughout England. When examined, eleven of the ova immediately became opaque on immersion in water. In the other eleven there was no loss of transparency, and in these, under the microscope, the circulation was found active. Those which had become opaque were placed in a pretty strong solution of common salt, by which their transparency was restored, the saline solution dissolving the coagulum. Now examined, no traces of development could be detected under the microscope in any one of them,—showing that they had been dead before they were sent away.

On the following day, the 18th of December, the eleven transparent ova were repacked, and again sent the same distance. They came back on the 22nd ; they retained then their transparency ; placed in water, a feeble circulation was to be seen in two under the microscope ; in nine the blood-corpuscles had ceased to flow ; these became opaque. Of the two in which the circulation was perceptible, one was hatched on the 28th of December ; the young fish in the other died, it would appear, in the act of breaking the membrane, its head, on the 29th, having been found protruding, but the heart's action stopped.

The fifth experiment was made on the 26th of December. Ten ova, in which the circulation was active, and the fœtus in each well advanced, were sent off on the day mentioned, and returned on the 31st. The weather, during the whole time, was mild, the frost having ceased. When opened, the ova were all found hatched, and the young fish dead, as might have been expected. When put into water, not one of them showed any signs of remaining vitality ; they were all examined under the microscope.

The sixth and last experiment was made on the 6th of January. Six ova, in each of which the circulation was vigorous, were put into a glass tube of one cubic inch and a half capacity, with water to the height of about 1·4 cubic inch, the remaining space, after closure by a cork, being filled with air. The intention was to try the effects of conveyance to a distance on these ova in water with a small quantity of air. Owing to a mistake, they were not forwarded. Examined on the following day, five ova were found hatched, the young fish dead ; in the one ovum remaining unhatched, the fœtus was alive, the circulation active ; on the 9th it burst its shell ; the young fish was vigorous.

As I could not with any certainty determine, at the time the experiments were commenced, what eggs were impregnated and alive, and what were not, I had at the beginning thirty ova taken indiscriminately from the common stock, and put apart in a glass vessel, the water in which was also changed daily. Of this number, seven were found in progress of development on the 14th of December, or 23 per cent.; the rest had become opaque. One of the seven was hatched on the 31st of December, the others in succession, the last on the 8th of January.

Further to arrive at a proximate average of the proportion of impregnated and unimpregnated ova, or living and dead, on the 14th of December, when in the living ova the circulation was distinct

under the microscope, and the embryos were visible even to the unaided eye, I examined the whole number then remaining, viz. 405, thus reduced, owing to 67 having been removed, one after another having become opaque, and 152 having been taken out for the purpose of experiments. Of these 405 remaining, 138 were found alive, each containing a well-formed embryo, and 267, though still transparent, without life, no marks of organization being to be seen in them, either with the naked eye or under the microscope. Hence, irrespective of the 152 experimented on, the proportion of living to dead on the 14th of December would appear to be as 138 to 364, or about 25 per cent. And, with the exception of two which died after the 14th, all those then alive were hatched, the first on the 31st of the same month, the last on the 9th of January.

What are the conclusions to be drawn from these results? From those of the first series of experiments, may it not be considered as proved that the power of resisting an undue increase of temperature is possessed in a higher degree by the ova in an advanced than in an early stage of development,—the degree probably being in the ratio of the age? From those of the second series, is it not as manifest that the power of bearing distant transport, and of retaining life in moist air, is in like degree increasing with age? And from both, may not the general conclusion be drawn, that the strength of vitality of the impregnated ovum, or its power of resisting agencies unfavourable to its life, gradually increases with age and the progress of fœtal development? And as the Charr is one of the most delicate of the family of fishes to which it belongs, may it not further be inferred, with tolerable confidence, that the ova of the other and more hardy species of the Salmonidæ, were they similarly experimented upon, would afford like results, confirmatory of those obtained last year in some trials on the ova of the Salmon, and mentioned in my former letter to you?

The practical application of these results, and of the conclusions deducible from them, is obvious, and need not at present be dwelt upon.

I am, my dear Sir, yours very truly,

Lesketh How, Ambleside,              JOHN DAVY.
    January 10, 1856.

"Note on a new Class of Alcohols." By M. Aug. Cahours and A. W. Hofmann, Ph.D., F.R.S. &c.

On submitting to dry distillation glycerine, either alone or together with bisulphate of potassium or anhydrous phosphoric acid, M. Redtenbacher obtained a remarkable product, to which he gave the name of acroleine. Presenting all the characters of an aldehyde, and approximating more particularly to vinic aldehyde by the general aspect of its reactions, this substance changes under the influence of oxidizing bodies, especially of oxide of silver, an acid being formed, named by this philosopher acrylic acid, an acid which stands in the same relation to acroleine as acetic acid does to aldehyde.

The researches of MM. Will and Wertheim on the essential oils of mustard and of garlic, tended to indicate a relation between these

substances on the one hand and acroleine and acrylic acid on the other, a result which was established by the more recent investigations of MM. Berthelot and De Luca. On studying the action of iodide of phosphorus on glycerine, these chemists obtained an iodine-compound named by them iodide of propylene, which is an analogue of the chloride and bromide of propylene, previously produced by MM. Cahours, Reynolds, and Hofmann, when submitting to the action of chlorine and bromine the gases which are formed when either amylic alcohol or valeric acid and its homologues are exposed to the influence of heat.

MM. Berthelot and De Luca have further shown that the result of the mutual decomposition of iodide of propylene and sulpho-cyanide of potassium is an oil identical with that obtained on distilling the seeds of black mustard with water in an alembic. By this remarkable experiment it is most clearly demonstrated that the volatile oil of mustard belongs to the propylene series, a relation which had been previously pointed out by Capt. Reynolds, but which he has omitted to establish by experiment. If, then, we admit the existence of a hydrocarbon, $C_6 H_5$, analogous to ethyle, $C_4 H_5$, we get

| | | | |
|---|---|---|---|
| $C_6 H_5 Cl$ | Chloride of propylene. | $C_4 H_5 Cl$ | Chloride of ethyle. |
| $C_6 H_5 Br$ | Bromide of propylene. | $C_4 H_5 Br$ | Bromide of ethyle. |
| $C_6 H_5 I$ | Iodide of propylene. | $C_4 H_5 I$ | Iodide of ethyle. |
| $C_6 H_5 S$ | Essential oil of garlic. | $C_4 H_5 S$ | Sulphide of ethyle. |
| $C_6 H_5 C_2 NS_2$ | Essential oil of mustard. | $C_4 H_5 C_2 NS_2$ | Sulphocyanide of ethyle. |
| $C_6 H_4 O_2$ | Acroleine. | $C_4 H_4 O_2$ | Aldehyde. |
| $C_6 H_4 O_4$ | Acrylic acid. | $C_4 H_4 O_4$ | Acetic acid. |

All that now remained was to discover the keystone of this edifice, in other words, to establish the existence of an alcohol to which the preceding compounds might be referred, and by the aid of which a still more numerous series of ethers, both simple and compound, and analogous in every respect to the derivatives of ordinary alcohol, might be obtained. After many protracted and unsuccessful attempts, we have succeeded in producing the alcohol and ether of this series, for which we propose retaining the name of the acryle series.

In order to arrive at this result, we have submitted several silver-salts to the action of iodide of acryle. There are but few acids whose salts lend themselves conveniently to this reaction. Among the various salts which we have examined with this view, the oxalate of silver has furnished the most satisfactory results. This salt is most violently attacked by iodide of acryle; the reaction is complete after two or three hours' digestion. The *oxalate of acryle* formed in this process, when separated from the iodide of silver, washed with water, dried over chloride of calcium, and redistilled, presents itself as a colourless transparent liquid, heavier than water, possessing a peculiar aromatic odour. It boils at 207°, and by analysis has been proved to contain
$$C_8 H_5 O_4 = C_2 (C_6 H_5) O_4.$$
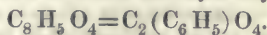
When treated with ammonia, oxalate of acryle furnishes oxamide and the alcohol, which was the object of our researches. This alcohol—*acrylic alcohol*—is a colourless transparent liquid of a peculiar, somewhat pungent odour, resembling that of mustard, and

which in fact is more or less characteristic of nearly all the members of the acryle series.

The analysis of acrylic alcohol has led to the formula

$$C_6 H_6 O_2 = 4 \text{ vols. of vapour.}$$
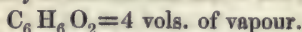
This compound is isomeric with acetone and with propyle-alde-hyde, from which substances, however, it differs essentially by the aggregate of its properties.

Acrylic alcohol burns with a much more luminous flame than ordinary alcohol. It mixes in all proportions with water. Treated with potassium, it disengages hydrogen and is converted into a trans-parent gelatinous mass, which is the *acryle-term corresponding to potassium-alcohol.*

This potassium compound is violently attacked by iodide of acryle ; a precipitate of iodide of potassium is thrown down, and a liquid is formed lighter than water, and insoluble in this fluid. This new substance corresponds to ordinary ether ; its formation is illustrated by the following equation :—

$$C_6 (H_5 K) O_2 + C_6 H_5 I = KI + C_{12} H_{10} O_2.$$
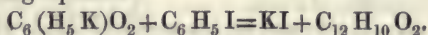
The same product is formed by the action of iodide of acryle upon oxide of silver or of mercury.

On treating the new potassium-alcohol with iodide of ethyle, or the ethyle-potassium-alcohol with iodide of acryle, an aromatic liquid is produced, which is obviously the *mixed ether of the ethyle and acryle series.*

If acrylic alcohol be distilled with chloride, bromide or iodide of phosphorus, the *chloride, bromide* and *iodide of the acryle series* are reproduced with the greatest facility.

Acrylic alcohol dissolves in concentrated sulphuric acid, without separation of carbon ; the liquid, mixed with water and neutralized with carbonate of barium, furnishes a crystalline salt, which contains

$$C_6 H_5 BaS_2 O_8 = BaSO_4, (C_6 H_5) SO_4.$$

This is the *sulphovinate of the series.*

On treating the mixture of acrylic alcohol with concentrated sulphuric acid, a most violent reaction takes place ; the alcohol is entirely carbonized with evolution of sulphurous acid.

Anhydrous phosphoric acid affects the alcohol with less energy. The mass darkens with evolution of a transparent colourless gas, burning with a luminous flame. The analysis of this gas remains to be made.

Acrylic alcohol is rapidly attacked by oxidizing agents. A mix-ture of sulphuric acid and bichromate of potassium acts with tre-mendous violence ; the products of the reaction being acroleine and acrylic acid, or its products of decomposition. The same transform-ation is effected by spongy platinum.

When treated with potassa and bisulphide of carbon, the new alco-hol solidifies at once into a mass of splendid yellow needles, which correspond to xanthate of potassium.

By the aid of the alcohol itself, its sulphovinic acid, or its iodide, all the terms of the acryle series may be produced with the greatest

facility.   We will specify the following compounds, the study of which we have more or less completed.

*Acryle-oxamethane*, or *oxamate of acryle*, is readily formed by adding alcoholic ammonia in small quantities to oxalate of acryle, until a permanent precipitate is produced.   The filtered solution deposits on evaporation the oxamate in magnificent crystals.

*Carbonate of acryle* is an aromatic oily liquid, lighter than water. It is formed like the other carbonic ethers, by the action of sodium upon the oxalate.   An alcoholic solution of this substance, when treated with baryta, furnished carbonate of barium and acrylic alcohol.

*Benzoate of acryle* is readily produced by the action of chloride of benzoyle upon acrylic alcohol.   It is a liquid heavier than water, which boils at 220°, and possesses an aromatic odour, similar to that of benzoic ether.   The analysis of this substance leads to the formula          $C_{20} H_{10} O_4 = C_{14} [H_5 (C_6 H_5)] O_4$.
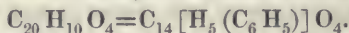
The same body is easily produced by the mutual reaction of iodide of acryle and benzoate of silver.

*Acetate of acryle*, obtained by the action of iodide of acryle upon acetate of silver, is a liquid lighter than water, of an odour resembling that of common acetic ether.   According to our analysis, it contains          $C_{10} H_8 O_4 = C_4 H_3 (C_6 H_5) O_4$.
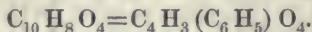
Cyanate of silver is most violently attacked by iodide of acryle, even in the cold.   The heat generated during this reaction is so powerful that the whole of the new product distils over.   The substance thus obtained has an incredibly penetrating odour, and causes lacrymation in the highest degree.   The analysis of this colourless transparent liquid, which boils at 82°, led to the formula

$$C_8 H_5 NO_2 = C_2 (C_6 H_5) NO_2.$$
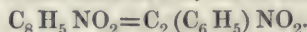
This is the *cyanate of acryle*. Gently warmed with a solution of ammonia, this liquid readily dissolves, and the solution deposits upon evaporation magnificent crystals, which are nothing but acrylic urea,

$$C_8 H_8 N_2 O_2 = C_2 H_3 (C_6 H_5) N_2 O_2,$$
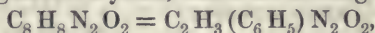
corresponding to thiosinnamine, the long-known sulphur-urea term of this series,          $C_8 H_8 N_2 S_2 = C_2 (H_3, C_6 H_5) N_2 S_2$.
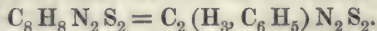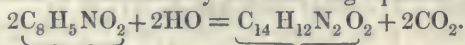
Aniline produces with cyanate of acryle an analogous substance, which crystallizes remarkably well.

When treated with water, cyanate of acryle is gradually converted into a solid crystalline substance.   The compound obtained in this manner has the composition and all the properties of *sinapoline* or *diacrylic urea*.   Its formula is

$$C_{14} H_{12} N_2 O_2 = C_2 [H_2 (C_6 H_5)_2] N_2 O_2,$$

and its formation is illustrated by the following equation :—

$$2\underbrace{C_8 H_5 NO_2}_{} + 2HO = \underbrace{C_{14} H_{12} N_2 O_2}_{} + 2CO_2.$$
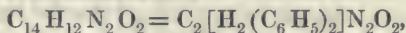
Cyanate of acryle. .          Sinapoline.

Cyanate of acryle is decomposed by a concentrated solution of potassa ; a solid substance is rapidly formed, which floats upon the surface of the solution, and which is nothing but the same sinapoline,

whilst a strongly alkaline liquid passes into the receiver, which is a mixture of several bases, in which we have traced already

(1) Methylamine.
(2) Propylamine.
(3) Acrylamine.

The latter substance boils between 180° and 190°. All our attempts to produce a well-crystallized platinum-salt of this base have hitherto failed.

The experiments detailed in the preceding sketch incontestably demonstrate the existence of a new series of alcohols, the third term of which is acrylic alcohol.

Like ordinary alcohol, this new alcohol furnishes a series of derivatives, which may be formulated in a similar manner.

The following tables exhibit the terms of the acryle series hitherto prepared in juxtaposition with the corresponding members of the ethyl series :—

| Acryle Terms. | | Ethyle Terms. |
|---|---|---|
| $C_6 H_6 O_2$ | Alcohol .......... | $C_4 H_6 O_2$ |
| $C_6 H_5 O$ or $C_{12} H_{10} O_2$ | } Ether ............ | { $C_4 H_5 O$ or $C_8 H_{10} O$ |
| $C_6 H_5 Cl$ | Chloride .......... | $C_4 H_5 Cl$ |
| $C_6 H_5 Br$ | Bromide .......... | $C_4 H_5 Br$ |
| $C_6 H_5 I$ | Iodide ............ | $C_4 H_5 I$ |
| $C_6 H_5 S$ or $C_{12} H_{10} S_2$ | } Sulphide .......... | { $C_4 H_5 S$ or $C_8 H_{10} S_2$ |
| $C_2 (K, C_6 H_5) S_4 O_2$ | Xanthate of potassium | $C_2 (K, C_4 H_5) S_4 O_2$ |
| $C_2 (C_6 H_5) NS_2$ | Sulphocyanide ...... | $C_2 (C_4 H_5) NS_2$ |
| $C_2 (C_6 H_5) NO_2$ | Oxycyanide or cyanate | $C_2 (C_4 H_5) NO_2$ |
| $C_2 (H_3 C_6 H_5) N_2 S_2$ | { Sulphuretted acryle-urea—Thiosinnamine } | ? |
| $C_2 (H_3, C_6 H_5) N_2 O_2$ | Acryle-urea, Ethyle-urea | $C_2 (H_3, C_4 H_5) N_2 O$ |
| $C_2 [H_2 (C_6 H_5)_2] N_2 O_2$ | { Diacryle-urea, Diethyle-sinapoline-urea...... } | $C_2 [H_2 (C_6 H_5)_2] N_2 O_2$ |
| $C_2 (C_6 H_5) O_4$ or $C_4 (C_6 H_5)_2 O_8$ | } Oxalate............ | { $C_2 (C_4 H_5) O_4$ or $C_4 (C_4 H_5)_2 O_8$ |
| $C_4 H_2 (C_6 H_5) O_4$ | Oxamate .......... | $C_4 (H_2, C_4 H_5) O_6$ |
| $C (C_6 H_5) O_3$ or $C_2 (C_6 H_5)_2 O_6$ | } Carbonate.......... | { $C (C_4 H_5) O_3$ or $C_2 (C_4 H_5)_2 O_6$ |
| $C_4 H_3 (C_6 H_5) O_4$ | Acetate............ | $C_4 (H_3, C_4 H_5) O_4$ |
| $C_{14} H_5 (C_6 H_5) O_4$ | Benzoate .......... | $C_{14} H_5 (C_4 H_5) O_4$ |
| $C_6 H_5 SO_4 HSO_4$ | Sulphovinic acid .... | $C_4 H_5 SO_4 HSO_4$ |
| $(C_6 H_5) H_2 N$ | { Acrylamine, Ethyl-amine ......... } | $(C_4 H_5) H_2 N$ |
| $C_6 H_4 O_2$ | { Aldehyde. Acrylic, Ethylic, Acroleine ......... } | $C_4 H_4 O_2$ |
| $C_6 H_4 O_4$ | { Acid. Acrylic ; acetic .... } | $C_4 H_4 O_4$ |
| $C_6 H_6$ | { Hydrocarbon. Propylene ?, Acetene } | $C_4 H_6$ |

Acrylic alcohol, the history of which we have endeavoured to sketch in the preceding pages, and in the study of which we are now engaged, is the third term of a series of alcohols, which is parallel to the ordinary alcohols of the formula

$$C_{n2} H_{n2+2} O_2,$$
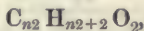
and the prototype of which is ethylic alcohol. The acid corresponding to this alcohol is acrylic acid, as has been stated. Chemists are already acquainted with several homologues of acrylic acid, which stand to the series of fatty acids in the same relation which exists between our new alcohol and common alcohol. Cyanide of acryle, which is readily procured by the action of iodide of acryle upon cyanide of silver, but which as yet we have not been able to obtain in a state of perfect purity, when submitted to the action of potassa, will obviously furnish an acid, homologous to acrylic acid equally as cyanide of propyle is transformed into butylic acid.

We terminate this note with a synoptical table of the two homologous groups.

| Group of Alcohols. | | Group of Acids, | | |
|---|---|---|---|---|
| $C_2 H_2 O_2$ | $C_2 H_4 O_2$ Methylic | $C_2 O_4$ | (Carbonic ?) | $C_2 H_2 O_4$ Formic |
| $C_4 H_4 O_2$ | $C_4 H_6 O_2$ Ethylic | $C_4 H_2 O_4$ | | $C_4 H_4 O_4$ Acetic |
| $C_6 H_6 O_2$ Acrylic | $C_6 H_8 O_2$ Propylic | $C_6 H_4 O_4$ Acrylic | | $C_6 H_6 O_4$ Propionic |
| $C_8 H_8 O_2$ | $C_8 H_{10} O_2$ Butylic | $C_8 H_6 O_4$ | | $C_8 H_8 O_4$ Butylic |
| $C_{10} H_{10} O_2$ | $C_{10} H_{12} O_2$ Amylic | $C_{10} H_8 O_4$ Angelic | | $C_{10} H_{10} O_4$ Valeric |
| $C_{12} H_{12} O_2$ | $C_{12} H_{14} O_2$ Caproic | $C_{12} H_{10} O_4$ | | $C_{12} H_{12} O_4$ Caproic |
| $C_{14} H_{14} O_2$ | $C_{14} H_{16} O_2$ | $C_{14} H_{12} O_4$ | | $C_{14} H_{14} O_4$ Œnanthylic |
| $C_{16} H_{16} O_2$ | $C_{16} H_{18} O_2$ Caprylic | $C_{16} H_{14} O_4$ | | $C_{16} H_{16} O_4$ Caprylic |
| $C_{36} H_{36} O_2$ | $C_{36} H_{38} O_2$ | $C_{36} H_{34} O_4$ Oleic | | $C_{36} H_{36} O_4$ Stearic |

This table exhibits a considerable number of gaps, which the progress of science will not be long in filling up. Even now we have established by experiment that bromide of amylene suffers many changes, which are perfectly analogous to those which we have witnessed in the acryle series, and even the derivatives of olefiant gas appear to exhibit in many respects an analogous deportment.

Feb. 21, 1856.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

"Account of the Observations and Computations made for the purpose of ascertaining the amount of the deflection of the Plumb-Line at Arthur's Seat, and the Mean Specific Gravity of the Earth." Communicated by Lieutenant-Colonel James, R.E., F.R.S. &c.

Col. James begins by observing, that as the Royal Society has, from the very commencement of the Ordnance Survey of the United Kingdom, taken a deep interest in its progress, he has great pleasure in announcing to the Society that all the computations connected with the Primary Triangulation, the measurement of the Arcs of Meridians and the determination of the figure and dimensions of the earth are now completed, and that the account of all the operations and calculations which have been undertaken and executed is now in the press, and will shortly be in the hands of the public.

In the progress of these operations it has been found, on determining the most probable spheroid from all the astronomical and geodetic amplitudes in Great Britain, that the plumb-line is consider-

ably deflected at several of the principal Trigonometrical Stations, and at almost every station the cause of the deflection is apparent in the configuration of the surrounding country.

The deflection of the plumb-line at Arthur's Seat is $5''\cdot1$, and at the Royal Observatory at Edinburgh it amounts to $5''\cdot63$ to the South. The unequal distribution of matter in the vicinity of these Stations—the great trough of the Firth of Forth being on the North and the range of the Pentland Hills on the South—presents an obvious cause for the deflection ; but as the contoured plans of the county of Edinburgh have been published and the best attainable data acquired for estimating the amount of local attraction at the above-mentioned stations, it appeared desirable specially to investigate the matter, both on account of its scientific interest and with a view to confirm the results arrived at from the previous investigation of all the observed latitudes. Col. James accordingly decided on having observations taken with Airy's Zenith Sector on the summit of Arthur's Seat, and at two other points near the meridian line on the North and South of that mountain, at about one-third of its altitude above the surrounding country.

The observations were made by Serjeant-Major Steel of the Royal Sappers and Miners, during the months of September and October last, and 220 double observations of stars were taken at each Station. The reductions and computations connected with these observations, as well as the computations of the local attraction at the Calton Hill, were entrusted to Captain Clarke, R.E., by whom the account now communicated of the mathematical investigation of the observed data has been drawn up.

Col. James has himself examined the geological structure of Arthur's Seat and of the whole of the county of Edinburgh, and has caused determinations to be made of the specific gravity of all the different rocks, with the view of estimating the mean specific gravity of the whole mass ; but he observes, that although the geological structure of Arthur's Seat is well exposed, and its mean specific gravity, $2\cdot75$, has been employed for deducing that of the earth, viz. $5\cdot1$, still it is not a mountain he should have selected for this special object. Accordingly he was pleased by discovering—on referring, since these observations were made, to the correspondence of the Survey,—that the late Dr. Macculloch had been employed from 1814 to 1819 in examining the whole of Scotland for the purpose of selecting a mountain which, from its homogeneous structure, size, and form, would be best suited for the purpose referred to, and that he had pointed out the Stack Mountain in Sutherlandshire as admirably answering the required conditions. The transfer of the whole force of the Survey from the North of England and Scotland to Ireland, prevented the late General Colby from undertaking this investigation ; but as the Survey of Scotland is now in full progress, Col. James purposes early in the spring to have the Stack Mountain and the surrounding country surveyed and contoured, and to have observations taken for determining the attraction of its mass, and he trusts by the close of the present year to lay the results before the Royal Society.

After these preliminary explanations, a detailed account is given of the mathematical investigation, and the paper, which was illustrated with plans and geological sections, and a model of Arthur's Seat, concludes with the following statement of the principal results :—

1. "The effect of the attraction of the Pentland Hills is observed in nearly equal amount at each of the three stations on Arthur's Seat.

2. "The calculated attractions of the mass of Arthur's Seat at the three stations are,—

| South Station. | Arthur's Seat. | North Station. |
|---|---|---|
| 2″·25 North. | 0″·34 South. | 1″·98 South. |

and, since the observed deflection at Arthur's Seat is 5″·27, the apparent effect of the Pentlands is 4″·93 at the summit of the hill.

3. "Of this deflection of 4″·93, the computed attraction due to the configuration of the ground within a radius of fifteen miles accounts for about 2″·6 ; and, inasmuch as we know that the igneous rocks of Arthur's Seat and the Pentland Hills have an origin at a great depth below the surface of the earth, the difference between the observed and computed attraction is probably owing in part to the high specific gravity of the mass of rock beneath them.

4. "The deflection at the Royal Observatory, Calton Hill, being 5″·63 South, exceeds that at Arthur's Seat by 0″·70. Of this deflection, 0″·60 is due to the configuration of the ground comprised within a circle of a mile and a quarter round the Observatory.

5. "The latitude of Arthur's Seat or points in the neighbourhood varies to the amount of 0″·02 between high and low water.

6. "The mean density of the earth, determined from the observations at the three stations on Arthur's Seat, is 5·14, with a probable error of ±·07 due to the probable errors of the astronomical amplitudes."

### CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xi. p. 405.]

Feb. 11, 1856.—A paper was read by Mr. Maxwell of Trinity College, on Faraday's Lines of Force, in continuation of a former paper (Phil. Mag. vol. xi. p. 404).

This paper was chiefly occupied with the extension of the method of lines of force to the phænomena of electro-magnetism, by means of a mathematical method founded on Faraday's idea of an "electro-tonic state."

In order to obtain a clear view of the phænomena to be explained, we must begin with some general definitions of *quantity* and *intensity* as applicable to electric currents and to magnetic induction. It was shown in the first part of this paper, that electrical and magnetic phænomena present a mathematical analogy to the case of a fluid whose steady motion is affected by certain moving forces and resistances. [The purely imaginary nature of this fluid has been

already insisted on.] Now the amount of fluid passing through any area in unit of time measures the *quantity* of action over this area ; and the moving forces which act on any element in order to overcome the resistance, represent the total *intensity* of action within the element.

In electric currents, the *quantity* of the current in any given direction is measured by the quantity of electricity which crosses a unit area perpendicularly to this direction ; and the intensity, by the resolved part of the whole electromotive forces acting in that direction. In a closed circuit, whose length $=l$, coefficient of resistance $=k$, and section $=C^2$, if F be the whole electromotive force round the circuit, and I the whole quantity of the current,

$$\frac{\text{I}}{\text{C}^2} lk = \text{F}, \quad \text{I} = \frac{\text{C}^2\text{F}}{lk}.$$

The laws of Ohm with respect to electric currents were then applied to cases in which the conducting power of the medium is different in different directions. The general equations were given and several cases solved.

In magnetic phænomena, the distinction of quantity and intensity is less obvious, though equally necessary. It is found, that what Faraday calls the quantity of inductive magnetic action over any surface, depends only on the *number* of lines of magnetic force which pass through it, and that the total electromotive effect on a conducting wire will always be the same, provided it moves across the same system of lines, in whatever manner it does so. But though the quantity of magnetic action over a given area depends only on the number of lines which cross it, the intensity depends on the force required to keep up the magnetism at that part of the medium ; and this will be measured by the product of the quantity of magnetization, multiplied by the coefficient of resistance to magnetic induction in that direction.

The equations which connect magnetic quantity and intensity are similar in form to those which were given for electric currents, and from them the laws of diamagnetic and magnecrystallic induction may be deduced and reduced to calculation.

We have next to consider the mutual action of magnets and electric currents. It follows from the laws of Ampère, that when a magnetic pole is in presence of a closed electric circuit, their mutual action will be the same as if a magnetized shell of given intensity had been in the place of the circuit and been bounded by it. From this it may be proved, that (1) the *potential* of a magnetized body on an electric circuit is measured by the *number* of lines of magnetic force due to the magnet which pass through the circuit. (2) That the total amount of work done on a unit magnetic pole during its passage round a closed curve embracing the circuit depends only on the *quantity* of the current, and not on the form of the path of the pole, or the nature or form of the conducting wire.

The first of these laws enables us to find the forces acting on an electric circuit in the magnetic field. Give the circuit any displace-

ment, either of translation, rotation, or disfigurement, then the difference of potential before and after displacement will represent the force urging the conductor in the direction of displacement. The force acting on any element of a conductor will be perpendicular to the plane of the current and the lines of magnetic force, and will be measured by the product of the quantities of electric and magnetic action into the sine of the angle between the direction of the electric and magnetic lines of force.

The second law enables us to determine the quantity and direction of the electric currents in any given magnetic field; for, in order to discover the quantity of electricity flowing through any closed curve, we have only to estimate the work done on a magnetic pole in passing round it. This leads to the following relations between $a_1$ $\beta_1$ $\gamma_1$, the components of magnetic intensity, and $a_2$ $b_2$ $c_2$, the resolved parts of the electric current at any point,

$$a_2 = \frac{d\beta_1}{dz} - \frac{d\gamma_1}{dy}, \quad b_2 = \frac{d\gamma_1}{dx} - \frac{d\alpha_1}{dz}, \quad c_2 = \frac{d\alpha_1}{dy} - \frac{d\beta_1}{dx}.$$

In this way the electric currents, if any exist, may be found when we know the magnetic state of the field. When $\alpha_1 dx + \beta_1 dy + \gamma_1 dz$ is a perfect differential, there will be no electric currents.

Since it is the *intensity* of the magnetic action which is immediately connected with the *quantity* of electric currents, it follows that the presence of paramagnetic bodies, like iron, will, by diminishing the total resistance to magnetic induction while the total intensity is constant, increase its quantity. Hence the increase of external effect due to the introduction of a core of soft iron into an electric helix.

From the researches of Faraday into the induction of electric currents by changes in the magnetic field, it appears that a conductor, in cutting the lines of magnetic force, experiences an electromotive force, tending to produce a current perpendicular to the lines of motion and of magnetic force, and depending on the number of lines cut by the conductor in its motion.

It follows that the total electromotive force in a closed circuit is measured by the *rate of change* of the number of lines of magnetic force which pass through it; and it is indifferent whether this change arises from a motion of this circuit, or from any change in the magnetic field itself, due to changes of intensity or position of magnets or electric currents.

This law, though it is sufficiently simple and general to render intelligible all the phænomena of induction in closed circuits, contains the somewhat artificial conception of the number of lines *passing through* the circuit, exerting a physical influence on it. It would be better if we could avoid, in the enunciation of the law, making the electromotive force in a conductor depend upon lines of force external to the conductor. Now the expressions which we obtained for the connexion between magnetism and electric currents supply us with the means of making the law of induced currents depend on the state of the conductor itself.

We have seen that from certain expressions for magnetic intensity we could deduce those for the quantity of currents, so that the currents which pass through a given closed curve may be measured by the total magnetic intensity round that curve. Here we have an integration *round the curve itself* instead of one *over the enclosed surface*. In the same way, if we assume the mathematical existence of a state, bearing the same relation to magnetic quantity that magnetic intensity bears to electric quantity, we shall have an expression for the quantity of magnetic induction passing through a closed circuit in terms of quantities depending on the circuit itself, and not on the enclosed space.

Let us therefore assume three functions of $x\, y\, z$, $\alpha_0\, \beta_0\, \gamma_0$, such that $a_1\, b_1\, c_1$ being the resolved parts of magnetic quantity,

$$a_1 = \frac{d\beta_0}{dz} - \frac{d\gamma_0}{dy}, \quad b_1 = \frac{d\gamma_0}{dx} - \frac{d\alpha_0}{dz}, \quad c_1 = \frac{d\alpha_0}{dy} - \frac{d\beta_0}{dx};$$

then it will appear that if we assume $\dfrac{d\alpha_0}{dt}, \dfrac{d\beta_0}{dt}, \dfrac{d\gamma_0}{dt}$ as the expressions for the electromotive forces at any point in the conductor, the total electromotive force in any circuit will be the same as that expressed by Faraday's law. Now as we know nothing of these inductive effects except in closed circuits, these expressions, which are true for closed currents, cannot be inconsistent with known phænomena, and may possibly be the symbolic representative of a real law of nature. Such a law was suspected by Faraday from the first, although, for want of direct experimental evidence, he abandoned his first conjecture of the existence of a new state or condition of matter. As, however, we have now shown that this state, as described by him (Exp. Res. (60.)), has at least a mathematical significance, we shall use it in mathematical investigations, and we shall call the three functions $\alpha_0, \beta_0, \gamma_0$, the *electrotonic functions* (see Faraday's Exp. Res. 60. 231. 242. 1114. 1661. 1729. 3172. 3269.).

That these functions are otherwise important may be shown from the fact, that we can express the potential of any closed current by the integral

$$\int \left( a_2\, \alpha_0 \frac{dx}{ds} + b_2 \beta_0 \frac{dy}{ds} + c_2 \gamma_0 \frac{dz}{ds} \right) ds,$$

and generally that of any system of currents in a conducting mass by the integral

$$\int \int \int (\alpha_0 a_2 + \beta_0 b_2 + \gamma_0 c_2)\, dx\, dy\, dz.$$

The method of employing these functions is exemplified in the case of a hollow conducting sphere revolving in a uniform magnetic field (see Faraday's Exp. Res. (160.)), and in that of a closed wire in the neighbourhood of another in which a variable current is kept up, and several general theorems relating to these functions are proved.

March 10.—A paper was read by L. Barrett, Esq., on the Distribution of the Mollusca on the Coast of Norway. (*Vide* Annals of Nat. Hist. May 1856.)

April 28.—Also, a paper was read " On the Question what is the Solution of a Differential Equation." By Professor De Morgan.

This paper is a short supplement to § 3 of a paper on some points of the integral calculus (Camb. Trans. vol. ix. part 2). It discusses the principles on which such an equation as $y'^2 = a^2$, giving

$$(y - ax + b)(y + ax + c) = 0,$$

is generally affirmed to be completely solved when $b = c$. It dwells on the distinction between a *relation* and an *equation*, which may express the alternative of one or more relations; it points out several cases in which conclusions applicable to the simple relation only are affirmed of any equation; and, with reference to the question asked in the title, discusses the manner in which the answer depends on the cross-question, what degree of discontinuity is allowed to be implied in the word *solution*?

May 12.—A paper was read by Mr. Warburton on Self-repeating Series, in continuation of a former paper.

The author showed in his former paper on self-repeating series, printed in vol. ix. part 4. of the 'Transactions' of the Society, that in the fraction which generates a series of either of the following forms,

$$1^{2n} \pm 2^{2n} \cdot t + 3^{2n} \cdot t^2 \pm \&c. \dots,$$

or

$$1^{2n+1} \pm 2^{2n+1} \cdot t + 3^{2n+1} t^2 \pm \&c. \dots,$$

the numerator of such fraction is a recurrent function of $t$. He also then determined the coefficients of the several powers of $t$ in such numerator to be given linear functions of the differences (as the case may be) of $0^{2n}$, or of $0^{2n+1}$.

In his present paper, from the $n$ pairs of equal coefficients which the recurrent numerator contains, the author obtains $n$ linear equations between the $2n$ differences concerned; and selecting any $n$ of these differences, he concludes that each of them can be expressed in terms of the other $n$ differences not so selected; and consequently that no formula, expressed in terms of the differences of $0^{2n}$ or $0^{2n+1}$, need contain more than $n$ of those differences.

He gives the equations requisite for obtaining $\Delta^{n+p}(0^{2n})$ in terms of $(\Delta^n, \Delta^{n-1}, \dots \Delta^2, \Delta^1)0^{2n}$; and $\Delta^{n+1+p}(0^{2n+1})$, in terms of $(\Delta^{n+1}, \Delta^n, \dots \Delta^3, \Delta^2)0^{2n+1}$; and he applies these and other of his equations to the elimination of particular differences of zero from sundry formulas.

Also, Mr. Bashforth exhibited models illustrating the Moon's motion.

## XL. *Intelligence and Miscellaneous Articles.*

### ON THE HEAT ABSORBED IN CHEMICAL DECOMPOSITIONS.

#### *To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,                                 Manchester, Sept. 15, 1856.

I AM sorry to have to trouble the patience of your readers on a matter personal between Dr. Woods and myself, but his letter in your last Number leaves me no alternative. My reply shall be as brief as possible.

Dr. Woods seems to be ignorant of the necessary and important distinction I have always drawn in my papers between resistance to conduction and resistance to electrolysis. I stated in my last, that I found that the heat evolved was in an electrolytic cell, as well as in a metallic conductor, proportional to the resistance to conduction and the square of the quantity (commonly called intensity) of current passed in a given time. I therefore still maintain, that his remarks in page 74 involve a mis-statement of my results. The law of which I adduced the example requoted by Dr. Woods, shows clearly the position I had arrived at in 1841, for it proves that I was well aware that resistance to electrolysis reacts upon the intensity of the battery, so as to cause a loss of heat to the entire circuit for a given quantity of current passed. That the actual amount of this loss is equal to the heat evolved by the reverse operation of chemical union was proved, as I said in my last, in vol. xix. p. 276 of this Magazine. Dr. Woods next says, that I " published a paper in the Philosophical Magazine proving that the heat developed by a galvanic current is proportional to its intensity." I did no such thing. My law was, that the " heat which is produced by *any pair* is proportional to its intensity and the number of atoms which are electrolysed in it." He goes on to say, that when the current was passed through an electrolyte, I thought part of the intensity was used up in causing electrolysis, and that the remainder only was effective in producing heat. Of course the intensity or electromotive force of the battery is so used up ; and hence it arises (as I proved), that after a certain amount of battery action has taken place, a loss of heat evolved is observed, not in the electrolytic cell alone, but in the entire circuit, the amount of such loss being, as I proved, equal to the heat which would be produced by the reunion of the separated elements.

But Dr. Woods is mistaken if he thinks that my claim rests for support on this individual paper of 1841. He will find in the Memoirs of the Literary and Philosophical Society of Manchester for 1846, a paper of mine, " On the Heat evolved during the Electrolysis of Water," read before that Society in January 1843, in which, after distinguishing between various kinds of resistance to the electrical current, I remark, p. 103, " that in the resistance accompanied by chemical changes the heat due to its reaction is rendered latent, and is thus lost by the circuit ;" and p. 104, " that however we arrange the voltaic apparatus, and whatever cells of electrolysis

we include in the circuit, the whole caloric of the circuit is exactly accounted for by the whole of the chemical changes." The same thing is reiterated in p. 105, in which I state, that in the electrolysis of water a "transfer of latent heat, equal to $8°\cdot27$ per equivalent, takes place from the battery to the electrolytic cell."

I still maintain, therefore, and with additional proofs, that the law claimed by Dr. Woods was proved by me in my papers from 1841 downwards; and I now pass on to my paper of 1846, of which Dr. Woods says, "Has it been published exactly as it is in their hands? Perhaps the absorption of heat might not be shown in it at all." I have fortunately the means of rebutting the injurious insinuation conveyed by these words. I herewith send you the rough copy of the translation of my original manuscript which M. Mordacque, Professor of the French language in this town, was so good as to make me. You will observe at the head of it the attestation of M. Mordacque, that it was prepared by him for me in 1845*.

I observe that Dr. Woods, in endeavouring to clear himself of my second charge, viz. that he endeavours to support his own claims as an original discoverer by setting up one portion of my papers in contradiction to another, merely reiterates the erroneous supposition he advanced in p. 75. I have only therefore to refer to my last letter in reply. Who is right, I or he, I leave with full confidence to the determination of any one who will read my papers with candour.

I have the honour to remain, Gentlemen,

Yours very respectfully,

JAMES P. JOULE.

## ON A LAW OF ELECTRICAL HEAT.    BY DR. P. RIESS.

### *To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I was in hopes that my reply to the attack of Sir W. Snow Harris would have been met by a substantial, tangible rejoinder, namely an exact description of the experiments which Sir William regards as incompatible with the law of electrical heat established by me. He has, however, preferred to come forward, in the August Number of the Magazine, with some scattered remarks, which are partly inessential and partly unproved, and to produce testimonies which are to confute me. I will briefly examine these observations in the order in which they occur; but I must for the future take care, that objections which I leave unanswered are not on that account regarded as well founded.

The author complains of the tone and mode of expression which prevail in my short reply to his long attack. I will not appeal to the fact that my letter, like the present one, was written in German and translated in England; for if one expression appears strength-

---

* We have compared the French translation with the paper published in the Magazine for 1852, and find them to be exactly the same.—EDS. Phil. Mag.

ened in English, in return another is softened. But, then, does Sir William think that the proper tone is hit in his memoir, in which such expressions as "clumsy way," "commonplace and unsound objection," and "somewhat learned endeavour," are made use of against me? If so, I need no excuse for having adopted a tone which he thinks improper.

In reference to his memoir in the Philosophical Transactions for 1834, I have charged Sir W. Harris with want of the necessary knowledge. Such a charge must be put up with by every one who draws from experiments untenable results, which it would have been impossible for him to have done had he been acquainted with the works of Franklin, Æpinus, Coulomb, and Poisson.

I am compelled to assume, that, nineteen years ago, I demonstrated that law of electrical heat which is attacked by Sir William, and that it has not yet been refuted. This law is not one of the points of my electrical labours regarding which doubts have been raised; on the contrary, it has been accepted by all physicists who treat of electricity in detail, and has proved to be in complete agreement with all the experimental and theoretical investigations which have since been made. If Sir W. Snow Harris be in a position to show the untenability of this law, he will do good service to science.

I am not quite clear as to what the author aims at with the resistance in the battery. If Sir William supposes the resistance between glass and coating to be constant when the battery is enlarged, he produces no change in my formulæ, in which the sum of all constant resistances is indicated; if he regards this resistance as variable, he throws over his own law of electrical heat.

In the following remark, Sir William is partly right. In my letter it runs, it is "*nicht richtig*" (which corresponds with the English "not correct") that I ascribed the unsatisfactory experiments of Sir W. Harris to the "faulty arrangement" of his thermometer. I had before me my original German memoir of 1837 (which I also referred to), in which the censure pronounced cannot be applied to the thermometer, but it had entirely escaped me that in 1838, I had worked up this memoir in French, when I ascribed those experiments to the imperfect (*imparfaite*) construction of the thermometer. It is therefore clear that I attempted to account to myself for the failure of Harris' experiments in different ways at different times,—by the inexact mode of measuring the electricity, the faulty mode of discharging the battery, and the want of sensibility in the thermometer. Latterly the first two causes appeared so sufficient to me, that I forgot the third.

Sir William characterizes the heat-experiments of Kinnersley with the thermometer as "casual," "a mere coincidence," "quite a secondary affair," and asserts that the experiments came to no result, which must raise the astonishment of any one who has ever even glanced at Kinnersley's letter. Kinnersley invented his thermometer solely with the view of refuting an opinion of Franklin's regarding the electrical heat, and introduces it with the words, "it fully determines that controverted point, whether there be heat in the electric fire." He deduces from his experiments the important point, "Hence it ap-

pears, that the electric fire, though it has not sensible heat when in a state of rest, will, by its violent motion and the resistance it meets with, produce heat in other bodies when passing through them, provided they be small enough. A large quantity will pass through a large wire without producing any sensible heat ; when the same quantity passing through a very small one, being then confined to a narrower passage, the particles crowding closer together and meeting with greater resistance will make it red-hot and even melt it." The circumstance that Franklin regarded the experiments without the thermometer as more demonstrative than those made with it, cannot justify us in attributing to Kinnersley the object* of measuring " the explosive violence of the discharge."

Whether Sir William, when describing his thermometer, was or was not acquainted with Kinnersley's thermometer is of no consequence now, but it is otherwise when he describes his as a peculiar instrument, after he has become acquainted with Kinnersley's. Equally inexcusable is his claiming for himself (Phil. Mag., vol. xi. p. 358) a method of measuring electricity, although he must be aware that it was described by Haldane.

Of the "perfect accuracy" of his unit-jar Sir William is so entirely convinced, that he prophesies that I shall soon be compelled to recognize it, and binds himself down to an offer which I will not be so ungenerous as to accept. I believe that I can prove, from the quotation at the close of his letter, that Sir William has no correct conception of the mode of action of his unit-jar ; and I repeat, that, according to the theory of the Leyden jar prevailing at the present day, the measurement with the unit-jar is inexact. Experiments, therefore, which are to show the accuracy of the unit-jar, must demonstrate the incorrectness of the theory of the Leyden jar, or they remain valueless ; for no physicist will employ a method which is theoretically inaccurate, when he has a theoretically accurate method at his command.

In conclusion, Sir William Harris gives an assurance, which I read with some astonishment, that he had not the intention of personally affronting me. I must confess that I have not found " a personal affront " in his memoir, but a not quite proper mode of carrying on a scientific discussion.

<div style="text-align:center">I have the honour to be, Gentlemen,</div>

<div style="text-align:center">Your obedient Servant,</div>

Berlin, August 22, 1856.                                              P. Riess.

---

OBSERVATIONS ON OZONE, RELATING TO A RECENT PAPER OF M. CLOËZ†.  BY M. BINEAU.

It has long been known to every chemist, that, under very various influences, iodide of potassium accompanied by starch gives rise to a blue coloration, and no one can have supposed that the so-called *ozonometric paper* of M. Schönbein was only impressionable by ozonized oxygen. But M. Schönbein and most observers who, like

---

* With his thermometer.

myself, have employed his paper in experiments on the atmosphere, have adopted as the most probable opinion that which attributes to ozone the bluish colour produced in the natural atmosphere. The probability of this opinion does not appear to me to be invalidated by the results of M. Cloëz.

Thus,—1. At a distance from laboratories or workshops whence hyponitric acid is evolved, the existence of rutilant vapours in the atmosphere appears to me very improbable. Again, I do not believe that the nitric acid produced by storms remains long without being neutralized, either by ammonia, or by the lime of the calcareous particles which float in the air in most countries ; and especially I do not think that this acid exists beyond the drops of rain, the influence of which M. Schönbein has recommended to be carefully avoided. Moreover, the hypothesis of the coloration of iodized starch by the nitric acid existing in the air has already been discussed by M. Schönbein, in consequence of observations made by M. Heller at Vienna. The latter having observed the nitrification of potash in the air, attributed this effect, and also the spontaneous coloration of iodized starch-paper, to atmospheric nitric acid. But M. Schönbein ascertained that this acid, diffused in the air in sufficient quantity to redden litmus paper rapidly, acted but slowly upon paper with iodized starch ; from which he concluded, that if the natural air contains minute quantities of this acid insensible to litmus, they can have no action upon iodized paper.

2. M. Cloëz has observed the production of a blue colour in paper exposed to the emanations of aromatic plants ; but is it not the same with regard to phosphorus ? Do we not see the oxidation of this body accompanied by an appearance of ozone, when the too active excess of the phosphorized vapours does not destroy the effect first produced ? and do we there attribute the coloration of the paper to other bodies than ozone ?

3. If light be capable of giving moist air the property of acting upon ozonometric paper only in a closed space, this fact, whatever interest it may possess, has no bearing upon the usual meteorological observations relating to ozone. I may add, that the exposure of the paper to the sun has one incontestable inconvenience, that of increasing the fugacity of the colorations produced, by the too rapid volatilization of the iodine. Therefore I have always recommended the observers who have lent me their assistance to avoid, as a general rule, the influence of the direct solar rays.—*Comptes Rendus,* July 21, 1856, p. 162.

### NOTE ON THE ENDOSMOSE OF GASES. BY J. JAMIN.

Döbereiner once filled a cracked bell-glass, standing in water in a pneumatic trough, with hydrogen gas. Although the sides of the crack were very closely pressed together, the hydrogen filtered slowly through this fissure and became diffused in the atmosphere. What was remarkable in this experiment is, that during the issue of the hydrogen a partial vacuum was produced in the bell-glass, and that the water rose several inches above the exterior level. This interesting observation was not then carried any further, but M. Magnus

afterwards took it up, using designedly-cracked glasses, and demonstrated that if, on the one hand, the hydrogen escapes outwards, the air, on the other, travels in an opposite direction and penetrates into the glass. By placing over the apparatus an uncracked bell-glass, so as to circumscribe the exterior atmosphere, Magnus also ascertained that the pressure increased in the air whilst it diminished in the hydrogen. There were therefore here two gases in contact through a capillary passage, both of which traversed this communication in opposite directions so as to unite; and the hydrogen filtering more rapidly than the air, a difference of pressure was soon proved. This was, for gases, the phænomenon of endosmose which Dutrochet had detected in liquids.

Since this period other facts of the same kind have been added to those just referred to. Marianini having placed a soap-bubble inflated with air in carbonic acid gas, saw it increase in size at first, and then burst. Other experimenters have produced the endosmose of gases through porous vessels or thin partitions, and the general fact has been placed beyond all dispute, although no convenient process for exhibiting it has been discovered, and nothing has led to a suspicion of the remarkable energy with which it is produced. I have to lay before the Academy a certain means of proving this endosmose and studying it in all its details.

I take a porous vessel of unglazed porcelain intended for the Bunsen's batteries, wash it with alcohol, and after leaving it to dry for several days, lay on its surface a coat of collodion dissolved in æther, or gutta-percha in sulphuret of carbon. This coat must be very thin, and envelope the entire surface of the vessel with a very continuous and equal varnish. I close it with a cemented obturator, which gives passage to two tubes, of which one bears a stopcock, whilst the other is open at both ends, and about 3 metres in length. I fix the whole vertically on a support, when the apparatus exhibits the porous vessel at its upper part, and the two tubes descend vertically from it; the longer of these dips into a trough filled with water, and the other, which bears the stopcock, serves for the introduction of the gases to be studied. I now pass in a current of hydrogen, which circulates in the porous vessel, returns by the long tube, and escapes through the water surrounding its base. This current of gas must be very abundant, and be maintained for several minutes; the stopcock is then closed.

At this moment the pressure diminishes in the porous vessel, the level of the water rises with remarkable rapidity, and in twenty seconds it attains a maximum height which varies from 2 to 2·50 metres. The liquid soon sinks gradually, and in a few minutes the internal pressure has again become equal to that of the atmosphere.

In its intensity, and almost in its rapidity, this experiment is equal to that with cracked glasses; it shows that two unequal gaseous currents are established, and that the difference of pressure which is their consequence may exceed a fifth, and attain nearly a fourth of an atmosphere; and as, at the moment when the maximum is observed, the gases are already mixed, the effect perceived is inferior to that which would be produced if these gases had remained pure.

It will be easily understood that the inverse experiments might be made with equal success. The porous vase might be surrounded with hydrogen and filled with air, when the interior pressure would increase instead of diminish.

Almost all the gases which I have studied furnish the same manifestations with less energy, and it appeared to me that the pressure always diminished with the gas which filters best, and increased with that which is the worst to filter. These experiments, like all those on the endosmose of liquids, must be explained, as it appears to me, by the theory proposed by M. Magnus, which makes endosmose depend, in the first place, on an unequal power of filtration of the two liquids in presence; and secondly, on a special force which sets them in motion,—a force which may be an affinity, an electrical action, or even a simple molecular action.—*Comptes Rendus*, July 28, 1856, p. 234.

## METEOROLOGICAL OBSERVATIONS FOR AUG. 1856.

*Chiswick.*—August 1, 2. Slight haze: very hot. 3. Hot and sultry. 4. Overcast: very hot. 5. Cloudless and hot. 6. Cloudy: slight haze: very fine. 7. Clear: hot and sultry. 8. Cloudy: overcast: very fine: rain. 9. Slight rain. 10. Cloudy and fine. 11. Very fine: cloudy: rain. 12. Cloudy: very fine. 13. Very hot: heavy rain at night. 14. Cloudy and fine: rain. 15. Cloudy and fine. 16. Very fine: thunder, lightning and rain at night. 17. Rain at half-past eight A.M.: excessively heavy rain commenced, nearly an inch fell in one hour: cloudy at night. 18. Cloudy. 19. Overcast: rain. 20. Foggy: overcast: heavy rain at night. 21. Densely clouded: boisterous, with heavy clouds and showers. 22. Partially overcast: cloudy: very fine. 23. Fine. 24. Cloudy and fine. 25. Slight showers. 26. Very fine. 27. Uniformly overcast: very fine. 28. Cloudy and fine: rain. 29. Very fine. 30. Slight fog: very fine. 31. Very fine: rain at night.

| | |
|---|---|
| Mean temperature of the month | 63°·40 |
| Mean temperature of Aug. 1855 | 00 ·00 |
| Mean temperature of Aug. for the last thirty years | 61 ·97 |
| Average amount of rain in Aug. | 2·413 inches. |

*Boston.*—Aug. 1—3. Fine. 4. Cloudy. 5, 6. Fine. 7. Fine: thunder, lightning and rain A.M. and P.M. 8. Fine. 9. Cloudy: rain A.M. and P.M. 10. Cloudy. 11, 12. Fine. 13. Cloudy. 14. Cloudy: rain A.M. 15, 16. Fine. 17. Rain A.M. and P.M. 18. Cloudy: rain A.M. and F.M. 19. Cloudy. 20. Cloudy: rain A.M. and P.M. 21. Rain A.M. and P.M. 22. Cloudy: rain A.M. 23. Cloudy. 24. Rain. 25. Cloudy. 26. Cloudy: rain P.M. 27. Cloudy: rain A.M. 28. Cloudy: rain A.M. and P.M. 29—31. Cloudy.

*Sandwick Manse, Orkney.*—Aug. 1. Drizzle A.M.: fog P.M. 2. Clear, fine A.M.: clear P.M. 3. Bright A.M.: cloudy P.M. 4. Bright A.M.: clear, fine, aurora P.M. 5, 6. Bright A.M.: clear, fine P.M. 7. Bright A.M.: cloudy, fine P.M. 8. Cloudy A.M. and P.M. 9. Cloudy A.M.: cloudy, fine P.M. 10. Bright A.M.: cloudy P.M. 11. Drops A.M.: drizzle P.M. 12. Bright A.M.: thunder-showers P.M. 13. Clear A.M.: bright P.M. 14. Bright A.M.: vapour, fine P.M. 15. Damp A.M.: drizzle P.M. 16. Damp A.M.: clear P.M. 17—22. Cloudy A.M. and P.M. 23. Cloudy A.M.: cloudy, drops P.M. 24. Bright A.M.: cloudy P.M. 25. Rain A.M.: showers P.M. 26. Showers A.M.: drizzle, showers P.M. 27. Cloudy A.M. and P.M. 28. Rain A.M.: drops P.M. 29. Showers A.M.: drops P.M. 30. Clear A.M.: cloudy P.M. 31. Rain A.M.: clear, aurora P.M.

| | |
|---|---|
| Mean temperature of Aug. for previous twenty-nine years | 55°·03 |
| Mean temperature of this month | 53 ·22 |
| Mean temperature of Aug. 1855 | 56 ·10 |
| Average quantity of rain in Aug. for previous sixteen years | 3·01 inches. |

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at* CHISWICK, *near London; by Mr. Veall, at* BOSTON; *and by the Rev. C. Clouston, at Sandwick Manse,* ORKNEY.

| Days of Month. 1856. Aug. | Barometer | | | | | Thermometer | | | | | Wind | | | Rain | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Chiswick Max. | Chiswick Min. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 8¼ p.m. | Chiswick Max. | Chiswick Min. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 8¼ p.m. | Chiswick 1 p.m. | Boston | Orkney, Sandwick | Chiswick | Boston | Orkney, Sandwick |
| 1. | 30·149 | 30·093 | 29·60 | 30·03 | 30·09 | 88 | 56 | 72 | 61 | 56 | e. | s. | w. | … | … | ·09 |
| 2. | 30·052 | 30·029 | 29·50 | 30·13 | 30·17 | 92 | 52 | 72 | 62½ | 56 | e. | ne. | e. | … | … | … |
| 3. | 30·066 | 30·022 | 29·51 | 30·17 | 30·17 | 89 | 55 | 70 | 58 | 54 | e. | ne. | e. | … | … | … |
| 4. | 30·181 | 30·117 | 29·64 | 30·21 | 30·27 | 87 | 55 | 65 | 57 | 54 | e. | ne. | se. | … | … | … |
| 5. | 30·216 | 30·190 | 29·70 | 30·26 | 30·23 | 84 | 44 | 67·5 | 58 | 53 | ne. | ne. | ese. | … | … | … |
| 6. | 30·171 | 30·046 | 29·66 | 30·17 | 30·07 | 78 | 44 | 64 | 65 | 53 | e. | ne. | ne. | … | ·22 | … |
| 7. | 30·009 | 29·874 | 29·44 | 30·00 | 29·90 | 88 | 41 | 66 | 56 | 52 | w. | w. | ne. | … | … | … |
| ☽ 8. | 29·718 | 29·640 | 29·20 | 29·83 | 29·83 | 80 | 42 | 66 | 53½ | 48 | sw. | s. | ne. | ·08 | … | … |
| 9. | 29·808 | 29·750 | 29·23 | 29·86 | 29·92 | 75 | 54 | 67 | 52½ | 48 | sw. | w. | ne. | ·10 | ·53 | … |
| 10. | 29·841 | 29·774 | 29·26 | 29·94 | 29·96 | 84 | 55 | 72 | 55 | 49 | se. | w. | e. | … | … | ·11 |
| 11. | 29·831 | 29·787 | 29·22 | 29·80 | 29·61 | 87 | 54 | 69 | 53½ | 55½ | sw. | ssw. | calm | ·12 | … | ·28 |
| 12. | 29·960 | 29·910 | 29·33 | 29·67 | 29·76 | 80 | 47 | 70 | 63½ | 60 | sw. | ssw. | se. | … | … | ·01 |
| 13. | 29·959 | 29·888 | 29·42 | 29·81 | 29·73 | 85 | 50 | 69 | 64 | 57 | s. | ssw. | se. | ·66 | … | … |
| 14. | 29·899 | 29·749 | 29·27 | 29·67 | 29·59 | 75 | 54 | 64 | 64½ | 54 | sw. | sw. | w. | ·06 | … | ·04 |
| 15. | 29·978 | 29·911 | 29·30 | 29·60 | 29·72 | 78 | 40 | 65·5 | 56½ | 55 | sw. | sw. | nne. | … | … | … |
| ○ 16. | 29·945 | 29·674 | 29·43 | 29·87 | 29·95 | 78 | 46 | 64 | 53½ | 46 | se. | calm | ne. | ·22 | ·12 | ·01 |
| 17. | 29·533 | 29·422 | 29·17 | 29·94 | 29·92 | 70 | 55 | 62 | 50½ | 48½ | ne. | ne. | nne. | 1·12 | … | … |
| 18. | 29·430 | 29·406 | 29·08 | 29·87 | 29·84 | 62 | 52 | 56 | 50 | 49 | ne. | nne. | nne. | … | … | … |
| 19. | 29·619 | 29·426 | 29·17 | 29·84 | 29·83 | 67 | 56 | 62 | 50 | 48 | ne. | nne. | ne. | ·29 | ·73 | … |
| 20. | 29·553 | 29·316 | 28·83 | 29·84 | 29·82 | 67 | 55 | 66 | 49½ | 48½ | se. | nne. | nne. | ·45 | ·19 | … |
| 21. | 29·400 | 29·255 | 29·16 | 29·77 | 29·97 | 68 | 49 | 64·5 | 51½ | 50½ | s. | s. | calm | ·24 | ·66 | … |
| ☾ 22. | 29·984 | 29·574 | 29·60 | 29·88 | 29·78 | 68 | 39 | 61 | 53 | 50 | n. | n. | se. | … | ·51 | … |
| 23. | 30·128 | 30·085 | 29·50 | 29·99 | 29·78 | 66 | 46 | 56 | 53 | 52 | n. | n. | wsw. | … | … | ·03 |
| 24. | 30·090 | 30·040 | 29·38 | 29·71 | 29·70 | 69 | 54 | 62 | 55½ | 54 | sw. | s. | wsw. | ·02 | … | ·01 |
| 25. | 29·983 | 29·812 | 29·36 | 29·36 | 29·28 | 66 | 52 | 66·5 | 52½ | 45 | sw. | sw. | n. | ·02 | … | ·29 |
| 26. | 29·929 | 29·901 | 29·22 | 29·69 | 29·78 | 76 | 55 | 61 | 51 | 44½ | w. | nw. | sse. | … | … | ·05 |
| 27. | 29·824 | 29·755 | 29·32 | 29·77 | 29·75 | 77 | 53 | 64·5 | 50½ | 50 | w. | w. | calm | ·07 | ·02 | … |
| 28. | 29·846 | 29·773 | 29·30 | 29·56 | 29·42 | 71 | 52 | 59 | 50 | 49 | sw. | sw. | nw. | … | ·04 | … |
| 29. | 30·144 | 29·888 | 29·66 | 29·69 | 29·98 | 76 | 40 | 64 | 53 | 50½ | sw. | w. | se. | … | ·06 | ·12 |
| ● 30. | 30·151 | 29·967 | 29·40 | 29·99 | 29·75 | 74 | 54 | 58·5 | 52½ | 50½ | s. | s. | w. | … | … | ·18 |
| 31. | 29·931 | 29·874 | … | 29·60 | 29·67 | 77 | 40 | … | 53 | 51½ | sw. | w. | … | ·05 | … | ·18 |
| Mean. | 29·914 | 29·804 | 29·35 | 29·855 | 29·849 | 76·74 | 50·06 | 64·7 | 55·37 | 51·08 | | | | 3·50 | 3·08 | 1·40 |

XLI. *A Theory of the Composition of Colours on the Hypothesis of Undulations. By* Professor CHALLIS*.

RECENT experiments have added largely to the class of facts that relate to the composition of colours, but nothing, as far as I am aware, has been done towards explaining the facts by any theory of light. The undulatory theory, if it be true, must admit of this application. In a communication to the British Association, published in the Report of 1834 (p. 544), I gave explanations in accordance with that theory of some facts relating to the composition of the colours of the spectrum, so far as to establish a presumption that *an analogy exists between the composition of colours and the composition of small vibrations.* To this subject I propose now to recur.

There is a peculiarity about the class of facts to be explained which must first be noticed. Colour is a *sensation,* not a material existence. It is, however, certain that corresponding to every sensation of colour there must be definite physical circumstances. But experiment has proved that when the external circumstances are the same, the sensations are not always the same in different individuals. The difference consists for the most part in the absence in some of sensations perceived by the majority, as in cases of colour-blindness. Admitting, for the moment, the truth of the undulatory hypothesis of light, the undulations of the æther may be conceived to excite in the nerves of the eye vibrations which in general correspond to the exciting undulations, but, owing to peculiarity of organization, may in some cases deviate from this normal character; for it cannot be imagined

* Communicated by the Author.

that if the parts of the eye were acted upon by the ætherial undulations in exactly the same manner in two individuals, the sensations would be different.   Now the undulatory theory of light ought to be capable of giving a reason for every positive sensation perceived by a great number of persons, but is not required to account for the absence of the sensation, or for a different sensation, in a few persons; because it may be presumed that these exceptional cases are due to causes unconnected with the laws of ætherial undulations.

In the undulatory theory of light, the formula which expresses the physical circumstances corresponding to the sensation of a pure colour is the following,—

$$v = m \sin \frac{2\pi}{\lambda} (x - at + c),$$

in which $v$ is the velocity of a vibrating element of the æther at the distance $x$ measured parallel to the direction of the motion from a fixed origin, and at the time $t$ reckoned from a given epoch, $m$ is its maximum velocity, $a$ is the velocity of propagation of the undulations, $\frac{\lambda}{a}$ is the time of one vibration backwards and forwards, and $c$ is a constant which is given when the velocity of vibration is given at a given position and a given instant.   The theory assumes that the colour-sensation produced by a simple series of undulations depends only on $\lambda$.

Hence it may be theoretically inferred that the colours of a pure prismatic spectrum are uncompounded.   For experiment has proved that both the colour and the angle of refraction for a given angle of incidence depend, the substance being given, only on the value of $\lambda$; whence it necessarily follows from the above supposition that *a ray of definite refrangibility is uncompounded*.

This is so strict a deduction from the undulatory theory, that if it should be contradicted by facts, that theory fails *in limine*. Two kinds of facts have been thought to contradict it; one derived from the composition of colours, the other from phænomena of absorption.   The objections drawn from the former will be met in the course of this communication by showing that the phænomena of the composition of colours can be explained on the supposition that the colours of the spectrum are pure.   At present I will say a few words on the phænomena of absorption.

We owe to Sir David Brewster the remarkable discovery, that a ray of a pure spectrum, on entering certain absorbing media which do not allow of its free transmission, may give rise to rays of various colours; and to Professor Stokes the additional dis-

covery, that this effect is produced even by rays more refrangible than those of the visible spectrum. This property, without diminishing the intensity of the colours which are freely transmitted, causes a peculiar dispersion to take place at the surface of the medium, and from points contiguous to the surface, the quality and quantity of which may be modified by the thickness of the absorbing substance. Do these facts, it may be asked, prove that a ray of definite refrangibility is composed of rays of different colours? A good theory of light ought to give an answer to this question. Not being able to form the slightest conception how a theory of light which makes the vibrations of individual atoms of the æther the exponents of visible properties of light can be brought to bear on this point, I venture to state that the question may be answered if the undulatory theory be based on hydrodynamical principles. In the pages of this Journal I proved long since, by general hydrodynamical equations, that the function which, as stated above, expresses the properties of an uncompounded ray, is not arbitrary, nor due to the mode of disturbance, but arises out of relations of a compressible fluid to time and space, which are shown by the analysis to be independent of particular disturbances. Consequently any disturbance proper for producing light will in general give rise to rays of all refrangibilities. When a ray of the spectrum enters an absorbing medium whose atomic arrangement does not allow of its transmission, the ray-undulations must be broken up, and the æther suffers disturbance by the action of the atoms at rest on the undulations, as really as when atoms by their motion disturb the æther at rest. According to this view, the generation of new light in an absorbing medium does not prove that the intromitted ray was compounded, since there is no resolution of the original ray, but a new disturbance. Now I think that I am right in saying that Sir David Brewster, in his examination of spectra produced by absorbing media, received this new (epipolic) light in conjunction with light freely transmitted; and that, owing to this circumstance, the colour of the latter appeared modified. At least this inference seems to be justified by the experiments of M. Helmholtz, who proved that the modifying light is decomposable by the prism, and got rid of the effect of it by insulating as much as possible the freely transmitted light (see Phil. Mag. S. 4. vol. iv. Dec. 1852). But M. Helmholtz does not appear to be aware that the experiments he made to account for what Sir David Brewster saw, were proper for getting rid of dispersed light peculiar to absorbing media, which originates in the *absorbed* rays and varies according to the degree of absorption, and that the insulation of the rays transmitted by the media was successful in removing the dispersed light, just because these

Z 2

rays do not give rise to such dispersion. By taking into account that absorption generates in greater or less degree epipolic dispersion, all the phænomena witnessed by the two experimenters may, I think, be explained consistently with the principle that a ray of definite refrangibility is uncompounded. I proceed now with the theory of the composition of colours.

The analytical formula which expresses that a ray is compounded of two or more simple rays is the following :—

$$v = m \sin \frac{2\pi}{\lambda}(x - at + c) + m' \sin \frac{2\pi}{\lambda'}(x - at + c') + \&\text{c.}$$

Assuming now that composition of colours corresponds to composition of ætherial undulations, the following explanations are given by the undulatory hypothesis of the leading facts which observation has established respecting the composition of colours:—

(1) The general fact of the composition and resolution of colours is explained by the principle of the coexistence of small vibrations on which the above formula depends.

(2) The result of compounding any number of undulations for which $\lambda$ is the same, is a series of undulations expressed by the formula $V = M \sin \frac{2\pi}{\lambda}(x - at + C)$, in which V is the algebraic sum of the separate velocities, and M is a function of $m$, $m'$, &c., and of the phases $c$, $c'$, &c. of the component undulations. Accordingly it is found by experiment that the composition of rays of a given colour produces a compound ray of the same colour. The coefficient M, which involves $c$, $c'$, &c., must, however, be distinguished from the coefficient $m$ of a simple series of undulations.

(3) If the values of $v$ at a given time be represented by the ordinates of a curve of which the abscissæ are the values of $x$, this curve will in general cut the axis of $x$ in a great number of points with irregular intervals between them. When this is the case, the result of the composition of the different rays is *white* light, and the degree of whiteness is greater the greater the irregularity. There is here a strict analogy to sound-sensations. As sounds are not all musical, so light is not all coloured. It may be admitted, that as colour in a simple ray is due to regularity of wave-intervals, so in every instance of the production of colour, the sensation is due to some species of regularity of recurrence in the waves. It may also be remarked, that the irregularity to which whiteness is due exists whatever epoch be selected, and consequently that whiteness is independent of the phases of the component undulations. This is known to be the case from experience.

(4) The effect of compounding two simple colours is expressed

in this theory by the formula

$$v = m \sin\frac{2\pi}{\lambda}\,(x - at + c) + m' \sin\frac{2\pi}{\lambda'}\,(x - at + c').$$

Let

$$m' = m + \mu, \quad \frac{1}{L} = \frac{1}{2}\Big(\frac{1}{\lambda} + \frac{1}{\lambda'}\Big), \text{ and } \frac{1}{l} = \frac{1}{2}\Big(\frac{1}{\lambda} - \frac{1}{\lambda'}\Big).$$

Then, the time being given, the expression may be put under the form

$$v = 2m \sin\Big(\frac{2\pi x}{L} + c_1\Big)\cos\Big(\frac{2\pi x}{l} + c_2\Big) + \mu \sin\Big(\frac{2\pi x}{\lambda'} + c_3\Big).$$

At present we will leave out of consideration the term containing $\mu$. The other term taken by itself shows that the axis of $x$ will be cut by the curve at a series of points separated by the common interval L, which is an harmonic mean between $\lambda$ and $\lambda'$, and at another series of points separated by the common interval $l$. As the ratio of the greatest and least values of $\lambda$ for light is nearly that of 3 to 2, $l$ will be at least equal to 5L. Hence the second series of recurrences will always be slower than the first; and in case $\lambda'$ be not much larger than $\lambda$, they will be much slower. The following considerations will show that in this case this second trigonometrical factor will have little or no sensible effect on the quality of the compound light. It is known from experiment, that if a stream of light be interrupted at short intervals by breaks, the sensation of light is still continuous. Suppose, therefore, the parts of the curve contiguous to those points of its intersection with the axis of $x$ which depend on the second factor, to be suppressed; the remaining portions will have the quality of regularity of recurrence which is necessary to produce colour, and the colour will plainly correspond to the wavelength L, which is intermediate to $\lambda$ and $\lambda'$. The suppressed portions, not satisfying the condition of regularity of intervals, may cause a sensation of whiteness, and thus have the effect of diluting the colour. These theoretical deductions agree exactly with a law first given by Newton as a result of experiment, viz. that " if any two colours be mixed, which in the series of those generated by the prism are not too far distant from one another, they, by their mutual allay, compound that colour which in the said series appeareth in the midway between them." This law is confirmed by the experiments of M. Helmholtz. (Phil. Mag. S. 4. vol. iv. p. 532.)

According to the theory, the condition that $m = m'$ must be approximately fulfilled. The effect of the term containing $\mu$ will be to introduce irregularity of intervals, and therefore whiteness;

and if $\mu$ be considerable, the whiteness may overcome the sensation of colour.    Also it is evident, that, to produce the intermediate colour, the *quantities* of the two colours must be mixed in certain proportions.    The effect of an excess of one of the colours will be to alter the tint; but as the change of tint must be continuous, the only change that can occur is an approach of the compound colour to that of the rays which are in excess.    Speaking theoretically, the intermediate colour is most perfectly produced when the *number* of rays of each kind is the same, and when $m = m'$ for each pair of rays.

(5) The following passage occurs in Sir John Herschel's Treatise on Light (art. 516):—"Blue and yellow combined produce green.    The green thus arising is vivid and rich; and, when proper proportions of the elementary colours are used, no way to be distinguished from the prismatic green.    Nothing can be more striking, and even surprising, than the effect of mixing together a blue and yellow powder, or of covering a paper with blue and yellow lines drawn close together and alternating with each other.    The elementary tints totally disappear, and cannot even be recalled by the imagination.    One of the most marked facts in favour of the existence of three primary colours, and of the possibility of an analysis of white light distinct from that of the prism, is to see the prismatic green thus completely imitated by a mixture of adjacent rays totally distinct from it both in refrangibility and colour."    According to this statement, a mixture of blue and yellow powders has the same effect as a mixture of blue and yellow light; for in the second mode of making the experiment, it is clear that the eye receives a mixture of blue and yellow rays.    I have made this experiment in another manner, by placing one upon the other two pieces of blue and yellow ribbon, and holding them up to the light.    The resulting green is very decided, and is plainly due to a mixture of lights.    M. Helmholtz has established by his experiments, that the mixing of coloured substances produces effects different from those of mixing prismatic colours, and in particular that prismatic blue and yellow do not produce green, or only a greenish-white.    The existence of green, in however small a degree, is a phænomenon which the theory ought to account for, and which in fact it does account for in the manner explained in art. (4).    Why the green is so much less conspicuous than the white will be shortly considered.    At present it is important to remark, that the experiment above adduced proves that blue and yellow light may produce a *vivid* green.    Consequently it must be concluded that a difference of some kind distinct from tint exists between the blue and yellow of that experiment, and prismatic blue and yellow. Now the theory I am proposing involves a mathematical condition

which points to such a difference. It is possible that, in the prismatic colours, the condition $m = m'$ may be less exactly fulfilled than in the colours of substances. A probable reason may be given for such a circumstance. Sunlight is generated within the sun's atmosphere at a great depth below the exterior boundary, and the ray-undulations have consequently to traverse a medium which may absorb them in different degrees according to the value of $\lambda$. The absorption might stifle particular rays, and change the value of $m$ for others; and if one colour, as yellow, were transmitted more readily than the rest, the value of $m$ for this colour would be larger than for the others, supposing that originally $m$ was the same for all. Thus the excess of brightness in the yellow part of the spectrum may be owing, not only to a greater number of yellow rays, but also to greater *intrinsic* brightness of each yellow ray. Now the coloured light of substances, though derived from sunlight, is yet new light, because it is produced at their surfaces, or within a small depth below their surfaces, by disturbances of the solar undulations similar to those already described with reference to absorption, which is, in fact, a phænomenon of the like kind, differing only in degree. This terrestrial light, not having passed through a large extent of atmosphere, may satisfy the condition $m = m'$, and in this respect differ from solar light, for which the value of $\mu$ in a mixture of blue and yellow rays may be considerable. Such a difference would account for the more complete production of green by the blue and yellow of terrestrial substances, than by the blue and yellow of solar light.

Our theory shows that there would be no reason to conclude, from even a perfect imitation to the sense of green by a mixture of yellow and blue, that the green of the spectrum is a compound colour.

(6) "Red with green gives a yellow, which, less saturated, is paler than the simple yellow." "Green gives with indigo a bright blue, which, however, is much duller and whiter than that of the spectrum; with violet also it gives a bright blue." (Helmholtz, Phil. Mag. pp. 525 and 526.) These facts, which relate to the colours of the spectrum, are in accordance with the theory. The dullness and whiteness of the resulting colour is owing, not only to the difference of wave-lengths of the components, but also to difference in the intrinsic brightness of the individual rays. For it may be remarked, that in the combination of colours, the wave-lengths of which differ considerably, the result is more decidedly coloured in the above instances than in that of the mixture of yellow and blue, although in this combination the difference of wave-lengths is not so great as in the others. The excess of whiteness of the blue and yellow compound may there-

fore be ascribed to a large value of $\mu$, that is, to a large difference of intrinsic brightness in the two kinds of rays.

(7) The following is, according to this theory, the explanation of the complementary colours of the solar spectrum. We have seen that when the ratio of $\lambda$ to $\lambda'$ for two colours does not differ much from a ratio of equality, the result of combining the colours is an intermediate colour with a mixture of whiteness. In proportion as that ratio deviates more from a ratio of equality, the whiteness will increase, because those points of intersection of the representative curve which depend on the factor $\cos\left(\dfrac{2\pi x}{l}+c_2\right)$ in art. (4) will be brought nearer to each other and produce a greater amount of irregularity. Consequently there must be some ratio of $\lambda$ to $\lambda'$ for which the whiteness so predominates as to destroy all colour. When this ratio is reached, the colours become *complementary*. Since we are dealing with *sensations*, it is not too much to assume that sensations produced by undulations of the æther have some analogy to sensations produced by undulations of the air. Respecting musical sounds, we know that if $\lambda$ and $\lambda'$ for two such sounds differ very little from each other, the result of the combination is to the sense musical. On increasing the difference, *beats* begin to be heard, which correspond to incipient whiteness. On still further increasing the difference, we reach complete discord, or unmusical noise, answering to perfect whiteness. M. Helmholtz has given (Poggendorff's *Annalen*, vol. xciv.) the following interesting results respecting the ratio of the wave-lengths of complementary colours :—

| Colour. | Wave-length. | Complementary colour. | Wave-length. | Ratio of wave-lengths. |
|---------|--------------|-----------------------|--------------|------------------------|
| Red. | 2425 | Green-blue. | 1818 | 1·334 |
| Orange. | 2244 | Blue. | 1809 | 1·240 |
| Gold-yellow. | 2162 | Blue. | 1793 | 1·206 |
| Gold-yellow. | 2120 | Blue. | 1781 | 1·190 |
| Yellow. | 2095 | Indigo-blue. | 1716 | 1·221 |
| Yellow. | 2085 | Indigo-blue. | 1706 | 1·222 |
| Green-yellow. | 2082 | Violet. | 1600 | 1·301 |

These experimental results show that the ratio of the wavelengths of the complementary colours of the spectrum varies from about the ratio of 4 to 3 for red and green-blue, to about the ratio of 6 to 5 for gold-yellow and blue. The production of whiteness with a less difference of wave-lengths in one combination than in another would, according to this theory, be due to a greater difference of intrinsic brightness of the component rays. We have already given a probable reason for concluding that this difference may be greatest when *yellow*, the predominant colour

of sunlight, is one of the components. The explanation given by the theory of there being no prismatic colour complementary to green is, that there is no colour the ratio of whose wave-length to that of green can be expressed by sufficiently low numbers.

(8) Again, it may be remarked, that, in sound-sensations, the combination of waves whose lengths are in the ratio of 4 to 3 produces *harmony* ; whilst, according to this theory of colour-sensations, the same ratio of wave-lengths of yellow and blue results in whiteness, which is analogous to unmusical noise. There is, however, a remarkable fact bearing on this point which must be taken into consideration. On proceeding to still greater differences of wave-lengths, when we come to a combination of *red* and *violet* colours, the ratio of whose wave-lengths is nearly that of the low numbers 3 and 2, the result is *purple*, a decided colour, in which the eye seems to be able to distinguish the components, as the ear distinguishes the components of a harmony. The analogy to sound here returns, with the difference that light-harmony does not appear to have so wide a range as sound-harmony. Possibly rose colour may be a harmonious result from a mixture of wave-lengths of *red* and *blue* in the ratio of 4 to 3.

(9) The method of combining colours by a painted revolving disc, and that of receiving in the same direction colours transmitted through, and reflected from, plate-glass (Helmholtz, Phil. Mag. p. 530), are, theoretically speaking, methods of varying the relative intensities of the two kinds of rays, and the value of the difference $\mu$ for each pair. In the case of the revolving disc, in which different kinds of impulses act in *succession*, the degradation of the vibrations of the particles of the eye in the comparatively large interval between one kind of impulse and the next succeeding one, is equivalent to a difference in the values of $m$ and $m'$ for two kinds of impulse which take effect simultaneously. The amount of difference, or the value of $\mu$, depends on the velocity of the motion of the disc. It is probable, however, that successive impulses can in no case be exactly equivalent to simultaneous impulses, because in the former the individual rays admit of being combined so that the intervals between the impulses will be different for each pair. It is also to be observed, that in the other experiment, when the intensities of the two parcels of light are so adjusted that neither colour is predominant, the values of $m$ and $m'$ for the individual rays will generally be unequal, because the intrinsic intensity of a reflected ray is, *cæteris paribus*, less than that of a transmitted ray. Hence a tendency to whiteness in the compound may be expected.

Several other inferences drawn by M. Helmholtz from his experiments are in accordance with this theory. For instance, that the extreme colours of the spectrum, red and violet, cannot be com-

pletely imitated by any combination; that the theory of three primitive colours must be abandoned, the colours produced by the combinations of any three being not so pure and vivid as those of the spectrum; and that white can be obtained in an infinite number of ways without the eye being able to distinguish one white from another. In fact, the number and variety of the explanations of this class of phænomena given by the undulatory theory may be adduced as a confirmation of its truth. No other theory of light has made the least approach to such explanations.

Cambridge Observatory,
    October 10, 1856.

XLII. *On the Application of the Mechanical Theory of Heat to the Steam-engine.* By R. Clausius.

[Continued from p. 265.]

27. THE influence exercised by the difference of pressure in the boiler and cylinder upon the work performed has hitherto been most completely treated of by Pambour in his work on the *Théorie des Machines à Vapeur*. Before entering upon the subject myself, therefore, I may be allowed to state the most essential parts of his treatment, altering only the notation, and neglecting the magnitudes which have reference to friction. By this means it will be easier, on the one hand, to judge how far this treatment is no longer in accordance with our more recent knowledge of heat, and, on the other, to add to it the new method of treatment which, in my opinion, must be substituted for the former one.

28. The two laws which, as was before mentioned, were formerly very generally applied to steam, form the basis of Pambour's theory. The first of these is Watt's law, according to which the sum of the latent and sensible heat is constant. From this law it was concluded, that when a quantity of steam at its maximum density is enclosed within a surface impenetrable to heat, and the volume of the enclosing space is either increased or diminished, the steam will neither become over-heated nor partially condensed, but will remain precisely at its maximum density; and it was further assumed that this would take place quite independently of the manner in which the change of volume occurred, whether thereby the steam had, or had not, to overcome a pressure corresponding to its own expansive force. Pambour supposed that the steam in the cylinder of a steam-engine deported itself thus; and at the same time he did not assume that the particles of water, which in this case are mixed with the steam, could exert any appreciable influence.

Further, in order to establish a more accurate relation between

the volume and the temperature, or the volume and the pressure of steam at a maximum density, Pambour applied, secondly, Mariotte's and Gay-Lussac's laws to the same. If, with Gay-Lussac, we assume the volume of a kilogramme of steam at 100° C., and at its maximum density, to be 1696 cubic metres, and reflect that the corresponding pressure of one atmosphere amounts to 10,333 kilogrammes on every square metre, then from the above law we obtain the equation

$$v = 1 \cdot 696 \cdot \frac{10333}{p} \cdot \frac{273 + t}{273 + 100}, \quad \cdots \quad (28)$$

where, with reference to the same units, $v$ and $p$ represent the volume and the pressure corresponding to any other temperature $t$. Herein it is only necessary to substitute in place of $p$ the values given in the tension series in order to have, according to the above assumption, the proper volume for each temperature.

29. In order, however, to be able conveniently to calculate the value of the integral

$$\int p\,dv,$$

which plays an important part in the formula for the work done by a steam-engine, it was necessary to find the simplest possible formula between $v$ and $p$ alone.

If, by means of the ordinary empirical formulæ for $p$, the temperature $t$ were eliminated from the above equation, the results would prove to be too complicated; hence Pambour preferred forming a special empirical formula for this purpose, to which, according to the process of Navier, he gave the following general form :—

$$v = \frac{B}{b + p}, \quad \cdots \quad \cdots \quad (29)$$

wherein B and $b$ are constants. He then sought to determine these constants, so that the volumes calculated from this formula might agree as nearly as possible with those calculated from the foregoing one. As this could not be done with sufficient accuracy, however, for all the pressures which occur in steam-engines, he established two different formulæ for machines *with* and *without* condenser.

The first of these was

$$v = \frac{20000}{1200 + p}, \quad \cdots \quad \cdots \quad (29a)$$

which agrees best with the above formula (28) between $\frac{2}{5}$ and $3\frac{1}{2}$ atmospheres; but is also applicable for a somewhat wider interval, from about $\frac{1}{2}$ to 5 atmospheres.

The second, for machines without condensers, is

$$v = \frac{21232}{3020 + p}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (29b)$$

which is most correct between 2 and 5 atmospheres, though the range of its applicability extends from about $1\frac{1}{5}$ to 10 atmospheres.

30. The magnitudes dependent upon the dimensions of the steam-engine, which enter into the determination of the work, shall be here, somewhat differently from Pambour's method, represented in the following manner. Let $v'$ be the whole space left free to the vapour during a stroke in the piston, the vicious space being also included. Let the vicious space form a fractional part $\epsilon$ of the whole space, so that this space itself will be represented by $\epsilon v'$, and that described by the surface of the piston by $(1 - \epsilon)v'$. Further, let the part of the whole space left free to the vapour up to the moment of disconnecting the cylinder and boiler (also inclusive of vicious space) be represented by $ev'$. Consequently the space described by the surface of the piston during the entrance of the vapour will be expressed by $(e - \epsilon)v'$, and that described by the same during expansion will be $(1 - e)v'$.

In order to determine, in the next place, the amount of work done during the entrance of the vapour, the pressure acting in the cylinder during this time must be known. This is at any rate smaller than the pressure in the boiler, otherwise no influx of vapour could occur; but the magnitude of the difference cannot in general be stated; for it depends not only upon the construction of the engine, but also upon the engine-driver, how far he has opened the valve in the tube leading from the boiler, and with what velocity he drives the machine. These things being changed, the above difference may vary between wide limits. Further, the pressure in the cylinder need not be constant during the whole time of influx, because the velocity of the piston may vary, as well as the magnitude of the influx orifice left free by the valve or the slide.

With respect to the last circumstance, Pambour assumes that the mean pressure to be brought into calculation in determining the work may, with sufficient accuracy, be set equal to that which exists in the cylinder at the end of the influx, and at the moment of disconnexion from the boiler. Although I do not think it advisable to introduce such an assumption—which is only adopted for numerical calculation in the absence of more certain data—at once into the general formulæ, yet here, whilst explaining his theory, I must adopt his method.

Pambour determines the pressure existing in the cylinder at the moment of disconnexion by means of the relation, established

by him, between volume and pressure; assuming at the same time that the quantity of steam which passes from the boiler into the cylinder in a unit of time, and therefore the quantity which passes during a stroke of the piston, is known from special observations.   As before, we will represent by M the whole mass which enters the cylinder during a stroke, and by $m$ the vaporous part of the same.   As this mass, of which Pambour only considers the vaporous part, fills the space $ev'$ at the moment of disconnexion, we have, according to (29), the equation

$$ev' = \frac{m \cdot B}{b + p_2},$$

where $p_2$ represents the pressure at the same moment.   From this equation we deduce

$$p_2 = \frac{m \cdot B}{ev'} - b. \quad . \quad . \quad . \quad . \quad (30)$$

Multiplying this magnitude by $(e - \epsilon)v'$, the space described by the surface of the piston up to the same moment, we obtain the following expression for the first part of the work :—

$$W_1 = m \cdot B \cdot \frac{e - \epsilon}{e} - v'(e - \epsilon)b. \quad . \quad . \quad (31)$$

The law according to which the pressure changes during the expansion which now follows, is also given by equation (29).   If at any moment $v$ represents the variable volume, and $p$ the corresponding pressure, then

$$p = \frac{m \cdot B}{v} - b.$$

This expression must be substituted in the integral

$$\int p \, dv,$$

and the integration effected between the limits $v = ev'$ and $v = v'$; whence, as second part of the work, we obtain

$$W_2 = mB \cdot \log \frac{1}{e} - v'(1 - e)b. \quad . \quad . \quad (32)$$

In order to determine the negative work done by the reacting pressure during the descent of the piston, this reaction must itself be known.   Without at present inquiring into the relation which exists between the reaction and the pressure in the condenser, we will represent the mean reaction by $p_0$, so that the work done by it will be expressed by

$$W_3 = -v'(1 - \epsilon)p_0. \quad . \quad . \quad . \quad . \quad (33)$$

There yet remains the work necessary to convey the quantity M of liquid back again into the boiler.   Pambour has not sepa-

rately considered this work, but has included it in the friction of the machine.    As I have included it in my formulæ, however, in order to have the cycle of operations complete, I will also here add it for the sake of easier comparison.    As shown by equations (21) and (22), established in a former example, this work will on the whole be expressed by

$$W_4 = -M\sigma(p_1 - p_0), \quad \cdots \quad \cdots \quad (34)$$

where $p_1$ and $p_0$ respectively represent the pressures in the boiler and condenser.    This expression, it is true, is not quite correct for our present case, because by $p_0$ we do not understand the pressure in the condenser itself, but in the parts of the cylinder in communication with the condenser.    Nevertheless we will retain the expression in its present form, for owing to the smallness of $\sigma$, the whole expression has a value scarcely worth consideration; and the inaccuracy, being again small in comparison to the value of the expression itself, may with still greater impunity be disregarded.

By adding these four separate amounts of work together, we find the whole work done during the circular process to be

$$W' = mB\left(\frac{e-\epsilon}{e} + \log\frac{1}{e}\right) - v'(1-\epsilon)(b+p_0) - M\sigma(p_1-p_0). \quad (35)$$

31. If, lastly, we wish to refer the work to the unit of weight of vapour instead of to a single stroke, during which the quantity $m$ of vapour acts, we have only to divide the foregoing value by $m$.    We will put $l$ in place of the fraction $\dfrac{M}{m}$, which expresses the relation which the whole mass entering the cylinder bears to the vaporous part of the same, and whose value is consequently a little greater than unity; V in place of the fraction $\dfrac{v'}{m}$, or the whole space offered to the unit of weight of vapour in the cylinder; and W in place of the fraction $\dfrac{W'}{m}$, or the work corresponding to the unit of weight of vapour.    We thus obtain

$$W = B\left(\frac{e-\epsilon}{e} + \log\frac{1}{e}\right) - V(1-\epsilon)(b+p_0) - l\sigma(p_1-p_0). \quad (XII)$$

Only one term of this equation depends upon V, and it contains V as factor.    As this term is negative, it follows that the work which we can obtain from the unit of weight of vapour is, all other circumstances being the same, greatest when the volume offered to the vapour in the cylinder is smallest.    The least value of this volume, which we may approach more and more although we may never quite reach, is that which is found by assuming

that the machine goes so slowly, or that the influx canal is so wide, that the same pressure $p_1$ exists in the cylinder as in the boiler. This case therefore gives the maximum of work. If with equal influx of vapour the velocity of motion is greater, or with equal velocity of motion the influx of vapour is smaller, we obtain from the same quantity of vapour a less quantity of work.

32. Before we now proceed to consider connectedly the same series of processes according to the mechanical theory of heat, it will be best to submit one of the same, which requires especial investigation, to a separate treatment in order at once to establish the results which have reference thereto. I refer to the *entrance of vapour into the vicious space and into the cylinder, when it has there to overcome a smaller pressure than that with which it was forced out of the boiler*. In this investigation I can proceed according to the same principles as those which I have employed in a former memoir* when treating similar cases.

The vapour from the boiler first enters the vicious space, here compresses the vapour of small density which still remains from the former stroke, fills up the space thus becoming free, and then presses against the piston, which, in consequence of its assumed comparatively small charge, recedes so quickly that the vapour cannot follow it quickly enough to reach the same density in the cylinder as it had in the boiler.

If saturated vapour alone issued from the boiler, it must under such circumstances become over-heated in the cylinder, for the *vis viva* of the entering mass is here converted into heat; as the vapour, however, carries with it some finely divided drops of water, a part of the latter will be evaporated by the surplus heat, and thus the remaining vapour will be maintained in its saturated condition.

* " Ueber das Verhalten des Dampfes bei der Ausdehnung unter verschiedenen Umständen," Pogg. *Ann.* vol. lxxxii. p. 263. With reference to this memoir, and to a notice connected therewith, which appeared in the Philosophical Magazine, Helmholtz, in his report published in the *Fortschritten der Physik,* by the Physical Society of Berlin (years 1850 and 1851, p. 582), is of opinion that the principle is in many points incorrect. I have not, however, been able to understand the reasons he adduces in support of this opinion. Views are ascribed to me which I never held, and, on the other hand, theorems enunciated which I never disputed, and which, indeed, partially constitute the basis upon which my own researches in the mechanical theory of heat are founded; at the same time so great a generality is maintained throughout, that I found it impossible to recognize how far those views ought to follow from my words, and these theorems contradict my conclusions. I do not therefore feel myself called upon to defend my former researches against this censure. As the following development, however, rests precisely upon the same views which before served me, Helmholtz will probably again find the same inaccuracy of principle. In such a case I shall look forward to his objections, and request him merely to enter somewhat more specially into the subject.

We must now consider the following problem :—*Given*, first, *the initial condition of the whole mass under consideration, viz. that which was previously in the vicious space, as well as that more recently arrived from the boiler* ; secondly, *the magnitude of the work done by the pressure acting upon the piston during the entrance of the vapour* ; and thirdly, *the pressure in the cylinder at the moment of cutting off the same from the boiler : to determine how much of the mass in the cylinder at this moment is vaporous.*

33. Let $\mu$ be the whole mass in the vicious space before the entrance of the fresh vapour, and, for the sake of generality, let us suppose that the part $\mu_0$ of it is vaporous and the rest liquid. For the present let $p_0$ and $T_0$ represent respectively the pressure of this vapour and its corresponding absolute temperature, without implying, however, that these are exactly the same values as those which refer to the condenser. As before, $p_1$ and $T_1$ shall be the pressure and temperature in the boiler, M the mass issuing from the boiler into the cylinder, and $m_1$ the vaporous part of the latter. As we have already remarked, the pressure upon the piston during the entrance of the vapour need not be constant. The *mean* pressure $p'_1$ may be defined as that by which the space described by the surface of the piston, during the entrance of the vapour, must be multiplied in order to obtain the same work as is actually done with the variable pressure. Further, let $p_2$ and $T_2$ be the pressure and corresponding temperature in the cylinder at the moment of cutting it off from the boiler ; and lastly, $m_2$ the magnitude to be determined, that is to say, the vaporous part of the whole mass $M + \mu$ now in the cylinder.

To determine this magnitude, let us conceive the mass $M + \mu$ reduced in any manner to its original condition. For instance, thus : let the vaporous part $m_2$ be condensed in the cylinder by depressing the piston, whereby we shall suppose that the latter can also enter the vicious space. At the same time let heat be constantly withdrawn from the mass in such a manner that the temperature $T_2$ may remain constant. Then of the whole liquid mass, let the part M be forced back into the boiler, where it may assume its original temperature $T_1$. By this means the condition of the mass within the boiler is the same as it originally was, for of course it is of no importance whether precisely the *same* mass $m_1$, which was before vaporous, is again so now, or whether *another equally great* mass has taken its place. With respect to the remaining part $\mu$, let it be at first cooled in the liquid state from $T_2$ to $T_0$, and at this temperature let the part $\mu_0$ become vaporous, to do which the piston must recede so that this vapour can again occupy its original volume.

34. In this manner the mass $M + \mu$ has gone through a complete circular process, to which we may apply the theorem, that

the sum of all the quantities of heat received by the mass during a circular process must be equivalent to the whole amount of external work thereby performed.

The following quantities of heat have been successively consumed :—

(1) To raise the temperature of the mass M in the boiler from $T_2$ to $T_1$, and at the latter temperature to evaporate the part $m_1$,

$$m_1 r_1 + Mc(T_1 - T_2).$$

(2) To condense the part $m_2$ at the temperature $T_2$,

$$-m_2 r_2.$$

(3) To cool the part $\mu$ from $T_2$ to $T_0$,

$$-\mu c(T_2 - T_0).$$

(4) To evaporate the part $\mu_0$ at the temperature $T_0$,

$$m_0 r_0.$$

Hence the total quantity of heat,

$$Q = m_1 r_1 - m_2 r_2 + Mc(T_1 - T_2) + \mu_0 r_0 - \mu c(T_2 - T_0). \quad (36)$$

The quantities of work may be found as follows :—

(1) In order to find the space described by the surface of the piston during the entrance of the vapour, we know that at the end of that time the whole mass $M + \mu$ occupies the space

$$m_2 u_2 + (M + \mu)\sigma.$$

From this we must deduct the vicious space. As at the commencement, this was filled by the mass $\mu$, of which $\mu_0$ was vaporous, at the temperature $T_0$, its volume is

$$\mu_0 u_0 + \mu\sigma.$$

Deducting this from the foregoing magnitude, and multiplying the difference by the mean pressure $p'_1$, we have for the first amount of work,

$$(m_2 u_2 + M\sigma - \mu_0 u_0)p'_1.$$

(2) The work expended in condensing the mass $m_2$ is

$$-m_2 u_2 p_2.$$

(3) In forcing back the mass M into the boiler,

$$-M\sigma p_1.$$

And (4). In evaporating the part $\mu_0$,

$$\mu_0 u_0 p_0.$$

By adding these four magnitudes, we obtain for the whole work W the following expression :—

$$W = m_2 u_2(p'_1 - p_2) - M\sigma(p_1 - p'_1) - \mu_0 u_0(p_1' - p_0). \quad (37)$$

If in the equation (1), which was

$$Q = A \cdot W,$$

we substitute the values of Q and W thus found, and then bring the terms involving $m_2$ to one side of the equation, we have

$$m_2[r_2 + Au_2(p'_1 - p_2)] = m_1 r_1 + Mc(T_1 - T_2) + \mu_0 r_0 - \mu c(T_2 - T_0)$$
$$+ A\mu_0 u_0(p'_1 - p_0) + AM\sigma(p_1 - p'_1). \quad . \quad . \quad . \quad . \quad (XIII)$$

By means of this equation the magnitude $m_2$ is expressed in terms of other magnitudes, all of which are supposed to be given.

35. If the mean pressure $p'_1$ were considerably greater than the final pressure $p_2$, it might happen that the value of $m_2$ would be less than $m_1 + \mu_0$, which would denote that a part of the vapour originally present had become condensed. This would be the case, for instance, if we were to suppose that, during the time the vapour was entering the cylinder, the pressure there was nearly equal to that in the boiler, and that by the expansion of this vapour already in the cylinder, the pressure ultimately sunk to the smaller value $p_2$. On the contrary, if $p'_1$ were but a little greater, or indeed smaller than $p_2$, then for $m_2$ we should find a value greater than $m_1 + \mu_0$. The latter ought to be considered as the rule in steam-engines, and amongst others it holds for the special case of $p'_1 = p_2$ assumed by Pambour.

We have thus arrived at results which differ essentially from Pambour's views. Whilst he assumes that the two different kinds of expansion which successively take place in the steam-engine are governed by one and the same law, according to which the original quantity of vapour is neither increased nor diminished, but always remains exactly at its maximum density, we have found two different equations which point to different deportments. By the equation (XIII), fresh vapour must be produced by the first expansion during the entrance of the steam; and according to the equation (VII), a part of the then existing vapour must become condensed when the further expansion takes place, after disconnecting the cylinder and boiler, during which time the work done by the vapour corresponds to its full expansive force.

As these two opposite actions, consisting of an increase and a diminution of vapour, which must also exercise opposite influences on the work performed by the machine, partially cancel one another, the ultimate result may, under certain circumstances, be approximately the same as that to which Pambour's simpler assumption leads. We must not, however, on this account neglect to consider this difference when once established, especially if we wish to ascertain in what manner a change in the construction or driving of the steam-engine will affect the magnitude of its work.

· 36. According to what was said in § 8, we can easily determine the uncompensated transformation which occurs in the expansion by referring the integral contained in the equation

$$N = -\int \frac{dQ}{T}$$

to the several quantities of heat expressed in § 34.

The quantities of heat $m_1 r_1$, $-m_2 r_2$, and $\mu_0 r_0$ are imparted at the constant temperatures $T_1$, $T_2$, and $T_0$, so that these parts of the integral are, respectively,

$$\frac{m_1 r_1}{T_1}, \quad -\frac{m_2 r_2}{T_2}, \text{ and } \frac{\mu_0 r_0}{T_0}.$$

The parts of the integral arising from the quantities of heat $Mc(T_1 - T_2)$ and $\mu c(T_2 - T_0)$, are found, by the method adopted in § 23, to be

$$Mc \log \frac{T_1}{T_2} \text{ and } -\mu c \log \frac{T_2}{T_0}.$$

By putting the sum of these magnitudes in place of the above integral, we obtain for the uncompensated transformation the value

$$N = -\frac{m_1 r_1}{T_1} + \frac{m_2 r_2}{T_2} - Mc \log \frac{T_1}{T_2} - \frac{\mu_0 r_0}{T_0} + \mu c \log \frac{T_2}{T_0}. \quad . \quad (38)$$

37. We can now return to the complete circular process which occurs in an acting steam-engine, and consider the several parts of the same in the same manner as before.

The mass M, of which the part $m_1$ is vaporous and the rest liquid, issues from the boiler, where the pressure is supposed to be $p_1$, into the cylinder. As before, the mean pressure acting in the cylinder during this time shall be $p'_1$, and the final pressure $p_2$.

The vapour now expands until its pressure sinks from $p_2$ to a given value $p_3$, and consequently its temperature from $T_2$ to $T_3$.

After this the cylinder is put in communication with the condenser, where the pressure is $p_0$, and the piston returns through the whole of the space it has just described. When the motion is somewhat quick, the reaction which it now experiences will be somewhat greater than $p_0$; to distinguish it from the latter value, we will represent the mean reaction by $p'_0$.

Similarly, the pressure of the vapour which remains in the vicious space after the piston's motion is completed will not necessarily be equal either to $p_0$ or to $p'_0$, and must consequently be represented by another symbol $p''_0$. It may be greater or less than $p'_0$, according as the communication with the condenser is cut off somewhat before, or somewhat after the conclusion of the piston's motion ; for in the first case the vapour would be a little

further compressed, whereas in the latter case it would have time
to expand a little more by partially passing into the condenser.

Lastly, the mass M is conveyed back from the condenser into
the boiler, when, as before, the pressure $p_0$ acts favourably, and
the pressure $p_1$ has to be overcome.

38. The expressions for the amounts of work done in these
processes will be quite similar to those in the simpler case before
considered, except that a few simple changes in the indices of
the letters will have to be made, and the magnitudes which refer
to the vicious space will have to be added.    In this manner we
obtain the following equations.

For the time during which vapour passes into the cylinder, we
have, according to § 34,

$$W_1 = (m_2 u_2 + M\sigma - \mu_0 u''_0)p'_1,    .    .    .    (39)$$

where $u''_0$ is simply substituted for $u_0$.

By putting $M + \mu$ in place of M, we have, from equation (IX),
for the expansion from the pressure $p_2$ to the pressure $p_3$,    .

$$W_2 = m_3 u_3 p_3 - m_2 u_2 p_2 + \frac{1}{A}[m_2 r_2 - m_3 r_3 + (M+\mu)c(T_2 - T_3)]. (40)$$

For the return of the piston, during which time its surface tra-
verses the whole space occupied by the mass $M + \mu$ at the pres-
sure $p_3$, diminished by the vicious space $\mu_0 u''_0 + \mu\sigma$, we have

$$W_3 = -(m_3 u_3 + M\sigma - \mu_0 u''_0)p'_0.    .    .    .    (41)$$

Lastly, for the conveyance of the mass M back into the boiler,
we have

$$W_4 = -M\sigma(p_1 - p_0).    .    .    .    .    .    (42)$$

Consequently the whole work is

$$W' = \frac{1}{A}[m_2 r_2 - m_3 r_3 + (M+\mu)c(T_2 - T_3)] + m_2 u_2(p'_1 - p_2) \left.\begin{array}{c}\\\\\end{array}\right\} (43)$$
$$+ m_3 u_3(p_3 - p'_0) - M\sigma(p_1 - p'_1 + p'_0 - p_0) - \mu_0 u''_0(p'_1 - p'_0)$$

The masses $m_2$ and $m_3$ which are here involved, are given by
the equations (XIII) and (VII), if in the former we put $p''_0$ in
the place of $p_0$, and change the magnitudes $T_0$, $r_0$, and $u_0$ in a
similar manner, and in the latter we substitute $M + \mu$ for M.
Nevertheless, although it is possible to eliminate $m_2$ and $m_3$ by
means of these equations, I will here merely replace $m_2$ by its
value; it being more convenient in calculation to consider the
equation which thus results in connexion with the two before
obtained.   The following, therefore, is the most general form of
the system of equations which serve to determine the work of
the steam-engine :—

$$\left.\begin{aligned}
W' &= \frac{1}{A}\left[m_1 r_1 - m_3 r_3 + Mc(T_1 - T_3) + \mu_0 r''_0 - \mu c(T_3 - T''_0)\right] \\
&\quad + m_3 u_3(p_3 - p'_0) + \mu_0 u''_0(p'_0 - p''_0) - M\sigma(p'_0 - p_0) \\
m_2[r_2 + Au_2(p'_1 - p_2)] &= m_1 r_1 + Mc(T_1 - T_2) + \mu_0 r''_0 \\
&\quad - \mu c(T_2 - T''_0) + A\mu_0 u''_0(p'_1 - p''_0) + AM\sigma(p_1 - p'_1) \\
\frac{m_3 r_3}{T_3} &= \frac{m_2 r_2}{T_2} + (M + \mu)c \log\frac{T_2}{T_3}.
\end{aligned}\right\} \quad \text{(XIV)}$$

39. Before endeavouring to render these equations more convenient for application, it may not be without interest to show how, for an imperfect steam-engine, the same expressions may be arrived at by a method before alluded to, and opposite to the one just applied. In order to avoid prolixity in this digression, however, we will only consider two of the imperfections provided for in the above equations, viz. the presence of vicious space, and the existence of a smaller pressure in the cylinder than in the boiler during the time that the vapour is passing into the former. On the other hand, we shall assume the expansion to be complete, therefore $T_3 = T_0$, and the magnitudes $T_0$, $T'_0$, and $T''_0$ to be equal.

In this determination we shall have to employ the equation (2), to which we will give the following form:—

$$W' = \frac{1}{A}\left(Q_1 - T_0\int_0^{Q_1}\frac{dQ}{T}\right) - \frac{T_0}{A}N.$$

The first term on the right-hand side of this equation denotes the work which could be obtained from the employed quantity of heat $Q_1$, which in our case is represented by $m_1 r_1 + Mc(T_1 - T_0)$, did not two imperfections exist. This term has been already calculated in § 23, and found to be

$$\frac{1}{A}\left[m_1 r_1 + Mc(T_1 - T_0) - T_0\left(\frac{m_1 r_1}{T_1} + Mc \log\frac{T_1}{T_0}\right)\right].$$

The second term denotes the loss of work caused by those two imperfections. The magnitude $N$ contained therein has been calculated in § 36, and is represented by the expression in equation (38).

Substituting these two expressions in the foregoing equation, we have

$$W' = \frac{1}{A}\left[m_1 r_1 - \frac{T_0}{T_1}m_2 r_2 + Mc(T_1 - T_0) - (M + \mu)c T_0 \log\frac{T_2}{T_0} + \mu_0 r_0\right]. \quad (44)$$

That this equation actually coincides with the equations (XIV), may be easily seen by using the third in order to eliminate $m_3$ from the first, and then setting $T_3 = T_0 = T'_0 = T''_0$.

In the same manner we might make allowance for the loss of

work occasioned by incomplete expansion. To do so it would only be necessary to calculate the uncompensated transformation produced in the passage of the vapour from the cylinder to the condenser, and to include the same in N. By this calculation, which need not here be executed, we obtain precisely the expression for the work which is given in (XIV).

40. In order next to be able to apply the equations (XIV) to a numerical calculation, it will be necessary first to determine the magnitudes $p'_1$, $p'_0$, and $p''_0$ more precisely.

With respect to the manner in which the pressure in the cylinder varies during the entrance of the steam, no general law can be instituted, because the entrance canal is opened and closed in such a variety of ways in different machines. Hence no definite universal value can be found for the relation between the mean pressure $p'_1$, and the final pressure $p_2$, as long as the latter is strictly interpreted. Nevertheless this will be possible if the signification of $p_2$ be slightly changed.

The cylinder and boiler cannot of course be instantaneously disconnected; more or less time is always required to move the necessary valves or slides, and during this interval the vapour in the cylinder expands a little, because, the orifice being diminished, the steam which enters is less than that which corresponds to the velocity of the piston. In general, therefore, we may assume that at the end of this time the pressure is already somewhat smaller than the mean pressure $p'_1$.

But if, in calculation, instead of restricting ourselves to the *end* of the time necessary for closing the entrance canal, we allow ourselves a little freedom in fixing the time of disconnexion, we shall be able to obtain other values for $p_2$. We can imagine the point of time so chosen, that if, previously to the same, the whole mass M had entered, the pressure at that moment would have been precisely equal to the mean pressure calculated up to the same time. By thus substituting the more definite momentary disconnexion in place of the actual gradual one, we incur but an insignificant error, as far as the amount of work is concerned. We may therefore adopt this modification of Pambour's assumption, and set $p'_1 = p_2$, reserving, however, for special consideration in each particular case the proper determination, according to the existing circumstances, of the moment of disconnexion.

41. With respect to the reaction $p'_0$ at the return of the piston, it is evident that, under otherwise equal circumstances, the difference $p'_0 - p_0$ will be smaller the smaller $p_0$ is. In machines with a condenser, therefore, it will be smaller than in machines without a condenser, where $p_0$ is equal to one atmosphere. In locomotives, the most important machines without condensers, there is usually a particular circumstance tending to magnify

this difference. The steam, instead of being allowed to pass off into the atmosphere through a tube as short and wide as possible, is conducted into the chimney and there made to issue through a somewhat contracted blowpipe in order to create an artificial draft.

In this case an exact determination of the difference is essential to the accuracy of the result. In doing so, regard must be had to the fact, that in one and the same machine the difference is not constant, but dependent upon the velocity with which it works; and the law which governs this dependency must be ascertained. Into these considerations, and into the investigations which have already been made upon the subject, I will not here enter, however, because they do not concern the mechanical theory of heat.

In machines where the vapour from the cylinder is not thus employed, and particularly in machines with a condenser, $p'_0$ differs so little from $p_0$, and therefore can change so little with the working velocity, that it is sufficient for most investigations to assume a mean value for $p'_0$.

Seeing, further, that the magnitude $p_0$ occurs only in one term of the equations (XIV), which term involves the factor $\sigma$, it can have but a very small influence on the amount of work; so that without hesitation we may put, in place of $p_0$, the most probable value of $p'_0$.

As already mentioned, the pressure $p''_0$ in the vicious space may vary very much, according as the cylinder is cut off from the condenser before or after the end of the piston's motion. But here, again, in equation (XIV), this pressure, and the magnitudes dependent thereon, occur only in terms which involve the small factors $\mu$ and $\mu_0$; so that we may dispense with an accurate determination of this pressure, and rest satisfied with an approximate evaluation. In cases where no particular circumstances are present to cause $p''_0$ to differ essentially from $p'_0$, their difference, like the difference between $p'_0$ and $p_0$, may be neglected, and the most probable value of the mean reaction in the cylinder may be assumed as the common value of all the magnitudes. This value may be represented simply by $p_0$.

By introducing these simplifications, the equations (XIV) become

$$\left.\begin{aligned}
W' &= \frac{1}{A}\left[m_1 r_1 - m_3 r_3 + Mc(T_1 - T_3) + \mu_0 r_0 - \mu c(T_3 - T_0)\right] \\
&\quad + m_3 u_3 (p_3 - p_0) \\
m_2 r_2 &= m_1 r_1 + Mc(T_1 - T_2) + \mu_0 r_0 - \mu c(T_2 - T_0) + A\mu_0 u_0 (p_2 - p_0) \\
&\quad + AM\sigma(p_1 - p_2) \\
\frac{m_3 r_3}{T_3} &= \frac{m_2 r_2}{T_2} + (M + \mu) \log \frac{T_2}{T_3}.
\end{aligned}\right\} \quad \text{(XV)}$$

42. In these equations it is assumed that—besides the masses M, $m_1$, $\mu$, and $\mu_0$, of which the first two must be known from direct observation, and the last two may be approximately determined from the magnitude of the vicious space,—the four pressures, $p_1$, $p_2$, $p_3$, and $p_0$, or what amounts to the same, the four temperatures $T_1$, $T_2$, $T_3$, and $T_0$, are given. In practice, however, this condition is only partially fulfilled, so that in calculation we must have recourse to other data.

Of the four pressures, only two, $p_1$ and $p_0$, can be assumed as known. The first is given immediately by the manometer on the boiler, and the second may at least be approximately deduced from the indications of the manometer attached to the condenser. The two others, $p_2$ and $p_3$, are not given; but in their place we know the dimensions of the cylinder, and at what position of the piston the cylinder is cut off from the boiler. From these we may deduce the volumes occupied by the steam at the moment of disconnexion and at the end of the expansion, and these two volumes will then serve as data in place of the pressures $p_2$ and $p_3$.

We must now bring the equations into such a form that the calculation may be made by means of these data.

43. Let $v'$, as in the explanation of Pambour's theory, again be the whole space, including vicious space, set free during one stroke in the cylinder; $ev'$ the space set free up to the time of disconnexion from the boiler; and $\epsilon v'$ the vicious space. Then, according to what was before said, we have the following equations :—

$$m_2 u_2 + (M + \mu)\sigma = ev'$$
$$m_3 u_3 + (M + \mu)\sigma = v'$$
$$\mu_0 u_0 + \mu\sigma = \epsilon v'.$$

The magnitudes $\mu$ and $\sigma$ are both so small that we may at once neglect their product, so that the above become

$$\left. \begin{array}{l} m_2 u_2 = ev' - M\sigma \\ m_3 u_3 = v' - M\sigma \\ \mu_0 = \dfrac{\epsilon v'}{u_0} \end{array} \right\} \quad \ldots \quad \ldots \quad (45)$$

Further, according to equation (VI),

$$r = AT ug,$$

where, on account of its subsequent frequent occurrence, a single letter $g$ is introduced in place of the differential coefficient $\dfrac{dp}{dT}$. Accordingly, we may replace $r_2$ and $r_3$ by $u_2$ and $u_3$ in the above system of equations; and then, as the masses $m_2$ and $m_3$ will only occur in the products $m_2 u_2$ and $m_3 u_3$, we may substitute the values of the latter as given in the first two equations of (45).

Similarly, by means of the last of these equations, we may eliminate the mass $\mu_0$; and as to the other mass $\mu$, although it may be a little greater than $\mu_0$, yet the terms which contain it as factor are altogether so unimportant, that we may without hesitation give it the same value as we have found for $\mu_0$; in other words, for the numerical calculation we may give up the assumption, made for the sake of generality, that the mass in the vicious space is partially liquid and partially vaporous, and suppose that the mass in question consists entirely of vapour.

The substitutions here mentioned may be made in the general equations (XIV), as well as in the simplified equations (XV). As they present no difficulties, however, we will here limit ourselves to the last, in order to obtain the equations in a form convenient for numerical calculation.

After this change the equations become

$$\left.\begin{aligned}
W' &= \frac{m_1 r_1 + Mc(T_1 - T_3)}{A} - (v' - M\sigma)(T_3 g_3 - p_3 + p_0) + \epsilon v' \frac{r_0 - c(T_3 - T_0)}{Au_0} \\
(ev' - M\sigma)T_2 g_2 &= \frac{m_1 r_1 + Mc(T_1 - T_2)}{A} + \epsilon v'\left(\frac{r_0 - c(T_2 - T_0)}{Au_0} + p_2 - p_0\right) \\
&\quad + M\sigma(p_1 - p_2) \\
(v' - M\sigma)g_3 &= (ev' - M\sigma)\, g_2 + \left(M + \frac{\epsilon v'}{u_0}\right)\frac{c}{A}\log\frac{T_2}{T_3}.
\end{aligned}\right\} \text{(XVI)}$$

**44.** In order to refer these equations, which now express the work done in a stroke or by the quantity $m_1$ of vapour, to the unit of weight of vapour, we have to proceed in the same manner as when the equations (35) were changed into (XII); that is to say, we divide each of the three equations by $m_1$, and set

$$\frac{M}{m_1} = l, \quad \frac{v'}{m_1} = V, \text{ and } \frac{W'}{m_1} = W.$$

Hereby the equations become

$$\left.\begin{aligned}
W &= \frac{r_1 + lc(T_1 - T_3)}{A} - (V - l\sigma)(T_3 g_3 - p_3 + p_0) + \epsilon V \frac{r_0 - c(T_3 - T_0)}{Au_0} \\
(eV - l\sigma)T_2 g_2 &= \frac{r_1 + lc(T_1 - T_2)}{A} + \epsilon V\left(\frac{r_0 - c(T_2 - T_0)}{Au_0} + p_2 - p_0\right) + l\sigma(p_1 - p_2) \\
(V - l\sigma)g_3 &= (eV - l\sigma)g_2 + \left(l + \frac{\epsilon V}{u_0}\right)\frac{c}{A}\log\frac{T_2}{T_3}.
\end{aligned}\right\} \text{(XVII)}$$

**45.** These equations may be applied in the following manner to the calculation of the work. From the intensity of evaporation, supposed to be known, and from the velocity with which the machine is at the same time driven, we determine the volume V which corresponds to the unit of weight of vapour. By means of this value we calculate the temperature $T_2$ from the second

equation, afterwards the temperature $T_2$ from the third, and lastly, we employ all these to determine the work from the first equation.

In doing so, however, we encounter a peculiar difficulty. In order to calculate $T_2$ and $T_3$ from the two last equations, they ought in reality to be solved according to these temperatures. But they contain these temperatures not only explicitly, but implicitly, $p$ and $g$ being functions of the same. If, in order to eliminate these magnitudes, we were to replace $p$ by one of the ordinary empirical formulæ which express the pressure of a vapour as a function of its temperature, and $g$ by the differential coefficient of the same, the equations would become too complicated for further treatment. We might, it is true, like Pambour did, help ourselves by instituting new empirical formulæ more convenient for our purpose, which, if not true for all temperatures, would be correct enough between certain limits. Instead of here entering into such experiments, however, I will draw attention to another method, by which, although the calculation is somewhat tedious, the several parts of the same are capable of easy execution.

[To be continued.]

## XLIII. *Second Note on the Theory of Logarithms.* By A. CAYLEY, *Esq.**

THE theory of logarithms, as developed in my first note (Phil. Mag. April 1856), may be exhibited in a clearer light by considering, instead of $\log a + \log b - \log ab = \mathrm{E}\pi i$, the equation $\log \dfrac{b}{a} = \log b - \log a + \mathrm{E}\pi i$, a form which more readily enables the accounting *à priori* for the discontinuity in the value of E. Writing then for $b$, $a$ the complex values $x' + y'i$, $x + yi$, we have

$$\log \frac{x' + y'i}{x + yi} = \log (x' + y'i) - \log (x + yi) + \mathrm{E}\pi i;$$

where, according to the assumed definition of a logarithm,

$$\log (x + yi) = \log \sqrt{x^2 + y^2} + i \left( \tan^{-1} \frac{y}{x} + \epsilon \pi \right),$$

in which $\log \sqrt{x^2 + y^2}$ is the real logarithm of $\sqrt{x^2 + y^2}$, and $\tan^{-1} \dfrac{y}{x}$ is an arc between the limits $-\dfrac{\pi}{2}$, $+\dfrac{\pi}{2}$. The coefficient $\epsilon$ is equal to zero when $x$ is positive; but when $x$ is negative, then $\epsilon = +1$ or $-1$, according as $y$ is positive or negative,

*i. e.* we have

$$x \equiv +, \quad \epsilon = 0,$$
$$x \equiv -, \quad \epsilon = \pm 1 \equiv y.$$

And of course the other logarithms in the equation have an analogous signification.

Hence, attending to the equation

$$\tan^{-1} \beta - \tan^{-1} \alpha = \tan^{-1} \frac{\beta - \alpha}{1 + \alpha\beta} + \epsilon''' \pi,$$

where, when $1 + \alpha\beta$ is positive, $\epsilon'''$ is equal to zero; but when $1 + \alpha\beta$ is negative, $\epsilon'''$ is equal to $+1$ or $-1$, according as $\beta - \alpha$ is positive or negative, or what is the same thing ($\alpha$, $\beta$ being of opposite signs when $1 + \alpha\beta$ is negative),

$$1 + \alpha\beta \equiv +, \quad \epsilon''' = 0,$$
$$1 + \alpha\beta \equiv -, \quad \epsilon''' = \pm 1 \equiv \beta - \alpha \equiv \beta \equiv -\alpha,$$

we find

$$E = \epsilon - \epsilon' + \epsilon'' - \epsilon''',$$

where $\epsilon$, $\epsilon'$, $\epsilon''$, $\epsilon'''$ are defined by the conditions—

$$x \equiv +, \quad \epsilon = 0,$$
$$x \equiv -, \quad \epsilon = \pm 1 \equiv y,$$
$$x' \equiv +, \quad \epsilon' = 0,$$
$$x' \equiv -, \quad \epsilon' = \pm 1 \equiv y',$$
$$xx' + yy' \equiv +, \quad \epsilon'' = 0,$$
$$xx' + yy' \equiv -, \quad \epsilon'' = \pm 1 \equiv xy' - x'y,$$
$$1 + \frac{y}{x}\frac{y'}{x'} \equiv +, \quad \epsilon''' = 0,$$
$$1 + \frac{y}{x}\frac{y'}{x'} \equiv -, \quad \epsilon''' = \pm 1 \equiv \frac{y'}{x'} - \frac{y}{x} \equiv \frac{y'}{x'} \equiv -\frac{y}{x}.$$

Suppose, to fix the ideas, that $x$, $y$ are each of them positive, we have $\epsilon = 0$; and considering the several cases,—

1. $x' \equiv +, \quad y' \equiv +.$

Here $xx' + yy' \equiv +$, $1 + \frac{y}{x}\frac{y'}{x'} \equiv +$; and consequently not only $\epsilon' = 0$, but also $\epsilon'' = 0$, $\epsilon''' = 0$, and thence $E = 0$.

2. $x' \equiv +, \quad y' \equiv -.$

Here $\epsilon' = 0$. Moreover, $xx'$ being positive, $xx' + yy'$ and $1 + \frac{y}{x}\frac{y'}{x'}$ will have the same sign. If they are both positive, then $\epsilon'' = 0$,

$\epsilon''' = 0$; but if they are both negative, then

$$\epsilon'' = \pm 1 \equiv xy' - x'y \equiv \frac{y'}{x'} - \frac{y}{x}$$

(since $xx' \equiv +$) and $\epsilon''' = \pm 1 \equiv \frac{y'}{x'} - \frac{y}{x}$, *i. e.* $\epsilon'' = \epsilon'''$. Hence in either case we have $E = 0$.

3. $x' \equiv -, \quad y' \equiv +$.

Here $\epsilon' = \pm 1 \equiv y'$, *i. e.* $\epsilon' = 1$. Also $xx'$ being negative, $xx' + yy'$ and $1 + \frac{y}{x}\frac{y'}{x'}$ will have opposite signs. Suppose first $xx' + yy'$ is positive, then $\epsilon'' = 0$. And $1 + \frac{y}{x}\frac{y'}{x'}$ being negative, we have $\epsilon''' = \pm 1 \equiv \frac{y'}{x'} - \frac{y}{x} \equiv \frac{y'}{x'}$, *i. e.* $\epsilon''' = -1$. But if $xx' + yy'$ is negative, then $\epsilon'' = \pm 1 \equiv xy' - x'y \equiv -\left(\frac{y'}{x'} - \frac{y}{x}\right)$ (since $xx'$ is negative) $\equiv -\frac{y'}{x'}$, *i. e.* $\epsilon'' = +1$. And $1 + \frac{y}{x}\frac{y'}{x'}$ being positive, we have $\epsilon''' = 0$. Hence in each case $E = 0$.

4. $x' \equiv -, \quad y' \equiv -$.

Here $\epsilon' = \pm 1 \equiv y'$, *i. e.* $\epsilon' = -1$. Also $xx'$ and $yy'$ being each negative, $xx' + yy'$ will be negative, and therefore

$$\epsilon'' = \pm 1 \equiv xy' - x'y \equiv -\left(\frac{y'}{x'} - \frac{y}{x}\right);$$

*i. e.* if $\frac{y'}{x'} > \frac{y}{x}$, then $\epsilon'' = -1$; but if $\frac{y'}{x'} < \frac{y}{x}$, then $\epsilon'' = +1$. And $1 + \frac{y}{x}\frac{y'}{x'}$ being positive, $\epsilon''' = 0$. Hence if $\frac{y'}{x'} > \frac{y}{x}$, then $E = 1 - 1 = 0$; but if $\frac{y'}{x'} < \frac{y}{x}$, then $E = 1 + 1 = 2$.

Consider $(x, y)$ $(x', y')$ as the rectangular coordinates of two points A, A'. In the case which has been considered, the point A has been taken in the positive quadrant; and the preceding discussion shows that we have always $E = 0$, except in the case where the finite line AA' meets the negative portion of the axis of $x$, in which case we have $E = +2$. The same thing is
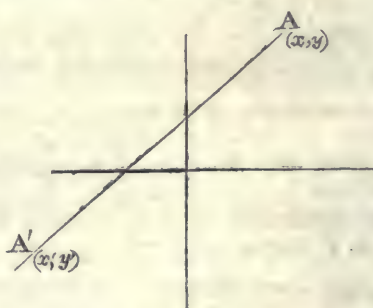
true generally in whichever quadrant A is situated, *i. e.* we have always E=0, except in the cases in which the finite line AA′ meets the negative portion of the axis of *x*. But when this happens, then if the line AA′, considered as drawn from A to A′, passes from above to below the axis of *x*, we have E= +2 ; but if the line AA′, considered as drawn from A to A′, passes from below to above the axis of *x*, then E= −2. So that treating the points A, A′ as the geometrical representations of the complex numbers $x+yi$, $x'+y'i$, we have in an exceedingly simple form the precise determination of the discontinuous number E(=0 or ±2) in the formula

$$\log\frac{x'+y'i}{x+yi} = \log (x'+y'i) - \log (x+yi) + \mathrm{E}\pi i.$$

Consider in general the definite integral

$$\int_z^{z'} \phi u \,.\, du,$$

where $z'$, $z$ are complex numbers of the form $x+yi$, $x'+y'i$; and take A, A′ as the geometrical representations of these limits, and the variable point P as the geometrical representation of the complex variable $u$. The value of the definite integral will depend to a certain extent on the series of values which we suppose $u$ successively to assume in passing from $z$ to $z'$, or what is the same thing, on the path of the variable point P from A to A′. For (excluding altogether the case in which the path passes through a point for which $\phi u$ becomes infinite) it is well known that the value of the definite integral is the same for any two paths which do not include between them a point for which $\phi u$ becomes infinite ; but when this condition is not satisfied, then the value of the definite integral is not in general the same for the two paths*. In order therefore to give a precise signification to the notations, we must fix the path of the point P, and it is natural to assume that the path is a right line
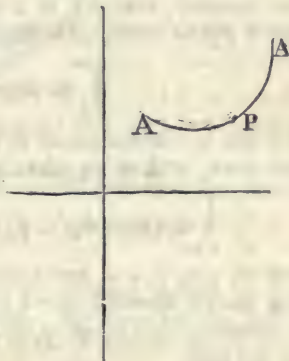
---

* The theorem is, I believe, due to M. Cauchy. See the memoir of M. Puiseux, *Recherches sur les Fonctions Algébriques*, Liouville, vol. xv, p. 365–480, where the subject is elaborately discussed.

(of course there are an infinity of paths which give the same value to the definite integral, or as we may call them, paths equivalent to the right line; but the consideration of these would be a needless complication of the definition, and it is better to attend to the single path—the right line). The definition is at once converted into an analytical one; we have only to assume $u = z + r(z' - z)$, and to suppose that the new variable $r$ passes from $r = 0$ (which gives $u = z$) *through real values* to $r = 1$ (which gives $u = z'$), *i. e.* we have as the equivalent analytical definition of the definite integral between the complex limits $z$, $z'$ the equation

$$\int_z^{z'} \phi u \, du = (z' - z) \int_0^1 \phi[z + r(z' - z)] \, dr,$$

where the new variable $r$ is real. The only restriction is, that $\phi u$ must not become infinite for any value of $u$ along the path in question, *i. e.* $\phi[z + r(z' - z)]$ must not become infinite for any real value of $r$ between the limits $r = 0$, $r = 1$.

Suppose next, the path being defined as above, or in any other manner, that $\phi_\prime u$ is a function of $u$ such that $\phi_\prime' u = \phi u$. Then if $\phi_\prime u$ is continuous along the entire path, we have

$$\int_z^{z'} \phi u \, du = \phi_\prime z' - \phi_\prime z;$$

but if $\phi_\prime u$ is discontinuous at any points of the path, *e. g.* at the point $u = u_\prime$, and at no other point, then

$$\int_z^{z'} \phi u \, du = \phi_\prime z' - \phi_\prime(u_\prime + \alpha') + \phi_\prime(u_\prime - \alpha) - \phi_\prime z,$$

where $u_\prime - \alpha_\prime$, $u_\prime + \alpha'$ are values indefinitely near to $u_\prime$, the path being from $z$ through $u_\prime - \alpha$ to $u_\prime + \alpha'$ and thence to $z'$. Or if we represent the break $\phi_\prime(u_\prime + \alpha') - \phi_\prime(u_\prime + \alpha)$ by the symbol $\nabla$, then we have

$$\int_z^{z'} \phi u \, du = \phi_\prime z' - \phi_\prime z - \nabla.$$

Suppose now

$$\zeta = \tan^{-1} \frac{y}{x} + \epsilon \pi,$$

where, as before, $\tan^{-1} \frac{y}{x}$ denotes an arc between the limits $-\dfrac{\pi}{2}$, $+\dfrac{\pi}{2}$, and

$$x \equiv +, \quad \epsilon = 0,$$
$$x \equiv -, \quad \epsilon = \pm 1 \equiv y;$$

and to fix the ideas, consider $\zeta$ as the $z$-coordinate of a surface,

the other two coordinates being $x$ and $y$. If $x$ be negative and $y$ be indefinitely small and positive, then $\epsilon = +1$, and we have $\zeta = \pi$; but if ($x$ being still negative) $y$ be indefinitely small and negative, then $\epsilon = -1$, and therefore $\zeta = -\pi$, *i. e.* there is a break or abrupt increment $2\pi$ of the coordinate $\zeta$ in passing across the negative part of the axis of $x$ from a negative to a positive value of $y$, or as we have before called it, from below to above; this is the only discontinuity in the surface, the form of the surface being, in fact, what is intended to be represented in the annexed figure.
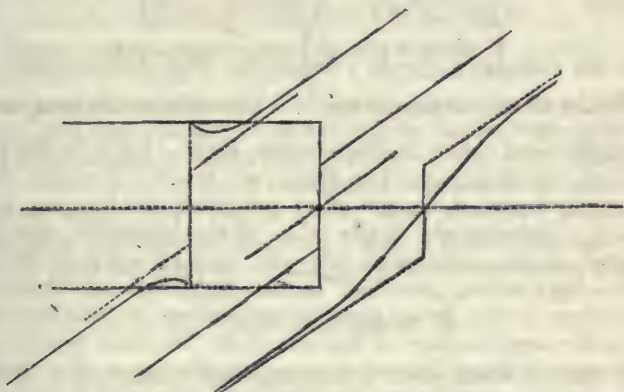


Suppose now $z' = x' + y'i$, $z = x + yi$, and consider the definite integral

$$\int_z^{z'} \frac{du}{u},$$

the path being, as before, a right line. We have by the equivalent analytical definition,

$$\int_z^{z'} \frac{du}{u} = (z' - z) \int_0^1 \frac{dr}{z + r(z' - z)},$$

where the new variable $r$ is real. And in like manner considering the integral

$$\int_1^{\frac{z'}{z}} \frac{du}{u},$$

the path being in this case also a right line, we have

$$\int_1^{\frac{z'}{z}} \frac{du}{u} = \left(\frac{z'}{z} - 1\right) \int_0^1 \frac{dr}{1 + r\left(\frac{z'}{z} - 1\right)},$$

where the new variable $r$ is real. The two integrals in $r$ are identically the same, and consequently we have in every case

$$\int_1^{\frac{z'}{z}} \frac{du}{u} = \int_z^{z'} \frac{du}{u}.$$

Now $\log' u = \dfrac{1}{u}$; and in passing from $u = 1$ to $u = \dfrac{z'}{z}$, there is no discontinuity in the value of $\log u$

$$= \log \sqrt{p^2 + q^2} + i \left( \tan^{-1} \frac{q}{p} + \epsilon \pi \right),$$

if for the moment $u = p + qi$; hence the value of the integral on the left-hand side is simply $\log \dfrac{z'}{z}$. The value of the integral on the right-hand side is in like manner, $\log z' - \log z$ in the case in which the finite right line from $u = z$ to $u = z'$ does not meet the negative part of the axis of $x$; but when this happens, then there is a discontinuity in the value of the logarithm, and the integral on the right-hand side will be $\log z' - \log z - 2\pi i$, or $\log z' - \log z + 2\pi i$, according as the right line considered as drawn from $z$ to $z'$ passes from below to above or from above to below the negative part of the axis of $x$. We have therefore in every case (E being defined as above) $\log z' - \log z + E\pi i$ for the value of the integral on the right-hand side, and the relation between the two integrals gives, as it ought to do, the equation

$$\log \frac{z'}{z} = \log z' - \log z + E\pi i,$$

or in the form in which it was before written,

$$\log \frac{x' + y'i}{x + yi} = \log (x' + y'i) - \log (x + yi) + E\pi i.$$

The preceding discussion shows that the discontinuity in the value of $E(-0$ or $\pm 2)$ arises from or is most intimately connected with the geometrical discontinuity which necessarily exists in the surface $z = \tan^{-1} \dfrac{y}{x}$, whenever we define the symbol $\tan^{-1}$ in such manner as to give a *unique* value to the coordinate $z$.

Stone Buildings,
Sept. 19, 1856.

XLIV. *On the Action of Nitric Acid on Alcohol at common Temperatures.* By Dr. H. DEBUS*.

NITROUS æther is decomposed, according to Berzelius, by contact with water, lime-water, or a solution of sulphate of iron, into saccharic or malic acid, nitric oxide, and other substances. Although the observation on which this assertion is founded was made fifty years ago, no contradiction of it seems to have become public, as it is still repeated in some of the latest handbooks on organic chemistry. The formation of saccharic acid from the radical æthyle appeared to me interesting enough to deserve a more minute examination, in order to determine the conditions on which it depends. I repeated carefully the experiments of Berzelius with pure hyponitrite of oxide of æthyle, water, and lime-water. In employing the latter, large quantities of nitrite of lime and traces of formiate of lime were produced. Pure water caused a great evolution of nitric oxide. After the decomposition was completed, the water contained nitric acid and very small quantities of an organic acid, which gave the reactions of formic acid. Similar results were obtained with sulphate of iron and nitrous æther. In no case was malic or saccharic, or any other organic acid except formic or oxalic acid, produced.

These facts are fully explained if it is assumed that nitrous æther is decomposed into alcohol and nitrous acid; the latter would form, according to circumstances, either nitrite of lime, or be converted into nitric oxide and nitric acid. A portion of the alcohol is oxidized to formic and oxalic acids.

The acid mother-liquor from which, according to Black's method, the hyponitrite of oxide of æthyle separates, is said to contain large quantities of saccharic acid. The examination of this liquid did not confirm this assertion, but led to other not less interesting results.

In order to obtain this liquid the following method was employed:—A narrow and tall bottle, of about 700 cubic centims. capacity, was charged with 220 grms. alcohol of 0·863 sp. gr. at 60° F., and put in a place where it could remain undisturbed during the period the experiment was going on. By means of a tube which had been drawn out to a narrow point at one end, 100 grms. of water were permitted to collect slowly under the alcohol, and 200 grms. of red fuming nitric acid under the water, in such a way that alcohol, water, and acid formed three distinct layers. The mouth of the bottle was then provided with a perforated cork, holding a bent glass tube, the other end of which dipped into a

small quantity of water. The temperature of the room was kept constant at 20° C. The nitric acid from one side and the alcohol from the other mix slowly with the water and meet each other. The alcohol is partly converted into nitrous æther, which escapes, and partly oxidized. An evolution of gas is observed, weak at the beginning, but strong at the end of the process. After the lapse of six or eight days, the three layers of water, alcohol, and nitric acid have completely mixed, the generation of gas ceases, and the operation is finished.

The liquid contains in this state, acetic, formic, oxalic, nitric, glycolic, and a new acid; also different kinds of æthers and aldehydes.

The mixture is evaporated on the water-bath, in quantities not exceeding 20 or 30 grms., till it assumes the consistency of strong syrup. If more than 30 grms. are evaporated at one time, then the substances to be prepared from it do not crystallize. Alcohol, nitrous æther, nitric, acetic, and formic acids, the aldehyde of acetic acid, and other volatile substances escape. Glycolic, oxalic, and the new acid, with a peculiar kind of aldehyde, remain in the evaporating basin. The contents of the latter are dissolved in a little water and neutralized with carbonate of lime. As soon as the effervescence has ceased, the liquid is mixed with an equal volume of strong alcohol. Nearly all the lime-salts are thrown down, whilst the aldehyde remains in solution. The precipitate is collected on a filter of linen, pressed, and then treated with a suitable quantity of boiling water. After filtering off from undissolved carbonate and oxalate of lime, the filtrate yields, on cooling and standing, a crystallization of a new compound of lime, which I propose to call glyoxylate of lime. On evaporating the mother-liquor, another quantity of the same substance may be obtained. If the liquid is further concentrated, a double salt of glycolate and glyoxylate of lime is separated. From the mother-liquor of the latter no more crystals can be prepared by evaporation. But if some slaked lime is added till it becomes alkaline, and it is boiled for a short time, filtered, and the excess of the lime removed by carbonic acid gas, it yields to the last drop crystals of glycolate of lime.

The liquid from which these lime-salts had been precipitated by alcohol leaves, after evaporation on the water-bath, a thick brown syrup, which is shaken with absolute alcohol. Except a small quantity of glycolate of lime, nearly the whole dissolves. The solution is filtered, and the alcohol distilled off at 100° C. A thick liquid remains in the retort, which belongs to the same class of compounds as the aldehyde of acetic acid; it unites with ammonia, oxide of lead, and bisulphite of soda. Of this substance I shall treat in another paper.

### Glyoxylate of Lime, $C^2 H^3 Ca O^4$*.

The crude salt is purified by repeated crystallization from boiling water. It forms small, hard prisms, which can be heated to 160° or 170° C. without undergoing any alteration in weight. They become yellow at 180° C., lose water and carbonic acid, whilst oxalate and glycolate of lime and a resinous substance remain. Exposed to a higher temperature on a piece of platinum foil, the smell of burning sugar is emitted, and carbonate of lime formed. One part dissolves in 177 parts of water at 8° C. The solution may be boiled without producing any decomposition of the glyoxylate of lime; it is precipitated by alcohol, and shows the following behaviour with reagents: nitrate of silver or baryta and chloride of copper do not produce a precipitate; acetate of lead throws down a white crystalline salt, which easily dissolves in acetic acid; lime-water acts in a similar manner. The precipitate is soluble in acetic acid immediately after its formation, but not after it has stood some time, or been exposed to a higher temperature. Oxalic acid separates the lime completely as oxalate of lime, and liberates the glyoxylic acid. A small quantity of solid glyoxylate of lime, heated with a solution of nitrate of silver and ammonia in a test-tube, produces a bright coating of silver on the glass.

Analysis gave the following results:—

I. 0·2165 grm., dried *in vacuo*, furnished 0·172 grm. $CO^2$ and 0·055 grm. water.

0·1945 grm. dissolved in water and precipitated by oxalate of ammonia gave, after treatment according to the usual method, 0·090 carbonate of lime.

II. 0·5175 grm. furnished 0·409 grm. $CO^2$ and 0·1265 HO.

0·405 grm. gave 0·183 grm. carbonate of lime.

III. 0·330 grm. gave 0·151 grm. carbonate of lime.

A small quantity of the substance burnt with soda-lime did not yield any ammonia. The residue did not contain a nitrogen compound. Another quantity of the same material was heated with potassium; no cyanide of potassium was produced. Therefore the glyoxylate of lime does not contain nitrogen.

In 100 parts,—

|          | I.    | II.   | III. |
|----------|-------|-------|------|
| Carbon   | 21·70 | 21·55 | ...  |
| Hydrogen | 2·80  | 2·71  | ...  |
| Calcium  | 18·50 | 18·07 | 18·3 |
| Oxygen   | ...   | ...   | ...  |

* C=12, H=1, Ca=20, O=16.

The formula $C^2 H^3 Ca O^4$ requires—

|          |   |   |   |   |    |        |
|----------|---|---|---|---|----|--------|
| Carbon   | . | . | . | 2 | 24 | 21·64  |
| Hydrogen | . | . | . | 3 | 3  | 2·70   |
| Calcium  | . | . | . | 1 | 20 | 18·01  |
| Oxygen   | . | . | . | 4 | 64 | 57·65  |
|          |   |   |   |   | 111 | 100·00 |

To a cold solution of glyoxylate of lime, lime-water was added till it became alkaline. After standing half an hour the white precipitate was filtered off, well washed, and treated with water and carbonic acid. The excess of the latter was removed by boiling. The precipitate now consisted of carbonate and oxalate of lime; the filtrate from it yielded, after evaporation, crystals of glyoxylate of lime.

0·185 grm., dried *in vacuo*, gave 0·085 carbonate of lime, or—

<div style="text-align:center">18·3 per cent. of calcium.</div>

The formula $C^2 H^3 Ca O^4$ requires—

<div style="text-align:center">18·01 per cent. of calcium.</div>

The filtrate from the precipitate by lime-water was also freed from its excess of lime by $CO^2$ and then evaporated; crystals of glycolate of lime were obtained.

0·162 grm., dried *in vacuo*, lost at 100° C. 0·035 water. The same quantity, dissolved and precipitated by oxalate of ammonia, gave 0·066 $Ca^2 O CO^2$. Or in 100 parts,—

|         |   |   |   |   |       |
|---------|---|---|---|---|-------|
| Water   | . | . | . | . | 21·6  |
| Calcium | . | . | . | . | 16·2  |

The formula $2(C^2 H^3 CaO^3) + 3H^2 O$ requires—

|         |   |   |   |   |       |
|---------|---|---|---|---|-------|
| Water   | . | . | . | . | 22·1  |
| Calcium | . | . | . | . | 16·3  |

A solution of glyoxylate of lime, made alkaline with lime-water and boiled for a short time, was decomposed into glycolate of lime and oxalate of lime. The same result was obtained when the cold mixture was allowed to stand a few days. Consequently, when lime-water is added to a cold solution of glyoxylate of lime, the latter combines with more lime and forms an almost insoluble substance. Slowly at common, but very fast at a high temperature, this compound resolves itself into glycolate and oxalate of lime and water,—

$$2C^2 H^3 Ca O^4 + Ca HO = C^2 Ca^2 O^4 + C^2 H^3 Ca O^3 + 2(H HO).$$
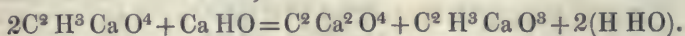
Sometimes I obtained the glyoxylate of lime crystallized, not in prisms, but in long needles. As the appearance of both was very different, I thought it necessary to examine them more minutely.

100 parts of water at 8° C. dissolved 0·56 of the prisms and 0·50 of the needles. Both solutions gave the same reactions with general reagents. 0·558 grm. of the prisms, boiled with a clear solution of caustic lime for a short time, then acidulated with acetic acid, and the oxalate of lime filtered off and converted by the usual method into carbonate of lime, yielded 0·254 of the latter substance. This corresponds to 0·325 of oxalate of lime.

0·391 grm. of the needles, treated in a similar manner, gave 0·176 $Ca^2 O CO^2$, or 0·225 oxalate of lime. Therefore from 100 parts—

$$\left\{ \begin{array}{l} \text{prisms } 58·2 \text{ oxalate of lime} \\ \text{needles } 57·5 \quad \cdots \end{array} \right.$$

If the decomposition takes place according to the equation—

$$2(C^2 H^3 Ca O^4) + Ca HO = \underbrace{C^2 Ca^2 O^4}_{\text{Oxalate of lime.}} + \underbrace{(C^2 H^3 Ca O^3)}_{\text{Glycolate of lime.}} + 2H^2O,$$
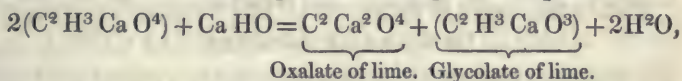
then 57·6 per cent. of oxalate of lime should have been found. These experiments show that both kinds of crystals belong to the same substance, and, moreover, they confirm the formula $C^2 H^3 Ca O^4$ for glyoxylate of lime.

The preparation of a double salt of glyoxylate and glycolate of lime was described in the beginning of this paper. It forms a white crystalline powder, more easily soluble in hot than in cold water. The warm saturated solution solidifies on cooling, forming a mass like gelatine, which after a few days' standing becomes crystalline.

Nitrate of silver, chloride of copper, acetate of lead, and limewater, give the same reactions with this substance as with glyoxylate of lime. By repeatedly dissolving the compound in hot water and recrystallizing it, it appears to be decomposed into $C^2 H^3 Ca O^4$ and $C^2 H^3 Ca O^3$.

Analysis gave the following results :—

0·246 grm., dried at 120° C., gave 0·110 grm. $Ca^2 O CO^2$.

0·148 grm., dried at 120° C., yielded 0·048 grm. water and 0·119 grm. $CO^2$. In 100 parts,—

| | |
|---|---|
| Carbon . . . | 21·92 |
| Hydrogen . . | 3·60 |
| Calcium . . . | 18·00 |

The formula $2(C^2 H^3 Ca O^4) + C^2 H^3 Ca O^3 + H HO$ requires—

| | |
|---|---|
| Carbon . . . | 21·49 |
| Hydrogen . . | 3·28 |
| Calcium . . . | 17·91 |
| Oxygen . . . | ··· |

0·642 grm., dried *in vacuo*, lost at 120° C. 0·0495 grm. water, corresponding to 7·71 per cent.

If the above formula is multiplied by two, and three atoms of water of crystallization added, 7·45 per cent. should be obtained according to calculation.

It is apparent that the composition of this substance, dried at 120° C., is nearly the same as that of glyoxylate of lime,—

$$3(C^2 H^3 Ca O^4) + 2H = C^6 H^{11} Ca^3 O^{12} = 2(C^2 H^3 Ca O^4) + C^2 H^3 Ca O^3 + H^2 O.$$

Glyoxylate of lime.

In order to check the analysis, the double salt was boiled with lime-water. It was decomposed, and its glyoxylate of lime converted into oxalate and glycolate of lime. 111 parts of glyoxylate of lime should yield 64 parts of oxalate or 50 parts of carbonate of lime. If, therefore, the formula

$$2\big(2(C^2 H^3 Ca O^4) + C^2 H^3 Ca O^3 + H^2 O\big) + 3H^2 O$$
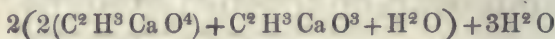
represents the composition of this compound, it should yield, after boiling with caustic lime, and conversion of the oxalate of lime into carbonate, 27·6 per cent. of the latter.

0·362 grm., dried *in vacuo*, gave 0·094 grm. Ca²O CO², or 26·2 per cent.

If the unstable character and the great difficulty of preparing this double salt in a pure state be taken into consideration, then both numbers seem to agree as well as could be expected.

## Glyoxylic Acid, $C^2 H^4 O^4$.

In order to obtain this acid, its lime-salt is dissolved in water and oxalic acid added as long as a precipitate of oxalate of lime is formed. After filtering, the clear liquid is evaporated *in vacuo*. The glyoxylic acid remains as a thick transparent syrup. It absorbs in this state moisture very rapidly from the air, and dissolves easily in water. The solution neutralizes potash and soda, and decomposes the carbonic acid compounds of all bases very rapidly. Oxide of silver is partly dissolved and partly reduced to metallic silver. The solution of glyoxylic acid, exposed in a retort to a temperature of 100° C., loses first water, and then the acid itself distils slowly over. It could not be obtained in a solid state. Heated in a test-tube by the flame of a common spirit-lamp, it boils, emits white fumes, and evaporates almost entirely without turning black.

## Glyoxylate of Potash, $C^2 H^3 Ka O^4$.

To glyoxylic acid is added a solution of carbonate of potash till about three quarters of the acid are neutralized. The liquid is then evaporated *in vacuo* until it yields with alcohol a copious

precipitate. As soon as this takes place, the whole of the solution is transferred to a glass cylinder and mixed with double its volume of strong alcohol. The compound required separated as an oily liquid, which soon solidified to a white crystalline mass.

Glyoxylate of potash is white, absorbs moisture from the air, and dissolves with great facility in water; it crystallizes only with difficulty in small prismatic crystals.

## *Glyoxylate of Ammonia,* $C^2 H^3 Am O^4$,

Is obtained by precipitating glyoxylate of lime with its equivalent quantity of oxalate of ammonia, and evaporating the filtrate from the oxalate of lime *in vacuo.* A hard crust is formed consisting of small white crystals.

By recrystallization it can be easily purified. The glyoxylate of ammonia dissolves slowly, but in large quantities, in water. The solution, especially when strong, is decomposed by boiling, and carbonic acid and a brown resinous substance formed. Alcohol dissolves very little of this salt, but does not precipitate its watery solution. The solid compound, heated on a piece of platinum foil, leaves a large quantity of coal, which burns only with difficulty.

Glyoxylate of silver could not be prepared in a pure state. A concentrated solution of it evaporated *in vacuo,* by absence of light, yields white crystals contaminated with metallic silver; the solution is neutral, and deposits a coating of silver on the glass when exposed to the light.

It appears that the glyoxylic acid contains two atoms of hydrogen which can be replaced by metals. The lime-salt unites with more lime and forms a compound nearly insoluble in water, which is decomposed by carbonic acid into carbonate of lime and glyoxylate of lime. A similar compound, containing baryta instead of lime, can be prepared. Both decompose very soon after their formation, and therefore could not be analysed. By adding acetate of lead to glyoxylate of lime, a white crystalline precipitate is thrown down. This precipitate dissolves easily in acetic acid and nitric acid; if heated in a test-tube with some liquid nitrate of silver and a few drops of ammonia, a bright coating of silver is formed on the glass.

0·2706 grm., dried at 100° C. and burnt with chromate of lead, gave 0·076 $CO^2$ and 0·0265 water.

0·165 grm. of the same substance yielded 0·117 metallic lead. In 100 parts,—

| | | | | | | |
|---|---|---|---|---|---|---|
| Carbon | : | . | . | . | . | 7·6 |
| Hydrogen | . | . | . | . | | 1·08 |
| Lead | . | . | . | . | . | 70·90 |
| Oxygen | . | . | . | . | . | ... |

The formula $C^2 H^2 Pb^2 O^4$ would require—

| | | | | |
|---|---|---|---|---|
| Carbon | . . . | 2 | 24 | 8·0 |
| Hydrogen | . . | 2 | 2 | 0·67 |
| Lead . | . . . | 2 | 207·6 | 69·7 |
| Oxygen | . . . | 4 | 64 | ... |
| | | | 297·6 | |

It contains therefore a little more lead than the calculation requires, a circumstance which occurs frequently with lead compounds, on account of their inclination to form basic salts.

Some experiments to substitute less than one atom of hydrogen in one atom of glyoxylic acid did not lead to any positive result. A certain quantity of the acid was divided into two equal parts; the one exactly saturated with carbonate of potash, and then the other mixed with it. After evaporating the liquid *in vacuo* a thick syrup remained, which could not be made to crystallize. On adding alcohol, glyoxylic acid was extracted, and an oily mass separated which soon became solid, and proved to be the salt $C^2 H^3 K O^4$.

0·284 grm., dried *in vacuo*, gave 0·162 chloride of potassium, corresponding to 30 per cent. of potassium. The formula $C^2 H^3 K O^4$ requires 30·2 per cent. A similar result was obtained with the lime-salt.

The action of nitric acid on alcohol and the formation of glyoxylic acid is explained in the following manner:—If from one atom of alcohol two atoms of hydrogen are deducted and two atoms of oxygen added, we obtain the formula of glycolic acid,—

$$\underbrace{C^2 H^6 O + 3O}_{\text{Alcohol.}} = \underbrace{C^2 H^4 O^3}_{\text{Glycolic acid.}} + \underbrace{H^2 O.}_{\text{Water.}}$$
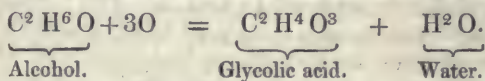
When water, alcohol, and nitric acid act upon each other at about 16° C., and as soon as these three substances have mixed, the liquid is treated as described, only glycolate of lime is obtained. When the same experiment was repeated at about 20° C., and especially when the mixture was allowed to stand for a few weeks after the action had apparently ceased, much glyoxylate of lime and less glycolate of lime was found. Therefore it seems as if the glyoxylic acid was formed at the expense of the glycolic acid,—

$$\underbrace{C^2 H^4 O^3}_{\text{Glycolic acid.}} + O = \underbrace{C^2 H^4 O^4.}_{\text{Glyoxylic acid.}}$$
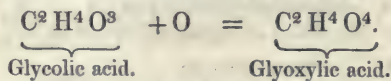
By allowing more time, or a higher temperature, the oxidation does not stop short at the production of $C^2 H^4 O^3$, but the latter takes up one atom of oxygen more, and becomes converted into

glyoxylic acid. In all cases small quantities of oxalic acid were also produced :—

$$\underbrace{C^2\ H^4\ O^4}_{\text{Glyoxylic acid.}} \ + \ O \ = \ \underbrace{C^2\ H^2\ O^4}_{\text{Oxalic acid.}} \ + \ \underbrace{H^2\ O}_{\text{Water.}}$$
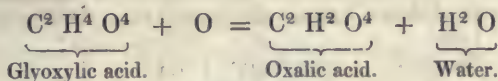
The great facility with which glyoxylic acid, under the influence of the strong bases, resolves itself into glycolic and oxalic acids and water, would lead to the view that it was composed of these three substances, and that its salts were merely double salts of oxalic and glycolic acids. The formula of the lime-salt would be accordingly—

$$\underbrace{\underbrace{C^2\ H\ Ca\ O^4}_{\text{Binoxalate of lime.}} \ + \ \underbrace{C^2\ H^3\ Ca\ O^3}_{\text{Glycolate of lime.}} \ + \ \underbrace{H^2\ O}_{\text{Water.}}}_{\text{Glyoxylate of lime,}}$$

or

$$\underbrace{\underbrace{C^2\ Ca\ Ca\ O^4}_{\text{Oxalate of lime.}} \ + \ \underbrace{C^2\ H^3\ H\ O^3}_{\text{Glycolic acid.}} \ + \ \underbrace{H^2\ O}_{\text{Water.}}}_{\text{Glyoxylate of lime.}}$$
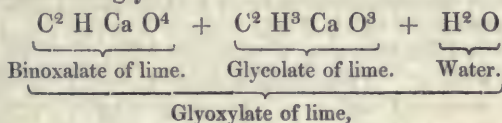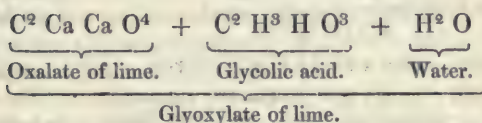
In order to decide this question, I tried to form glyoxylic acid from water, glycolic and oxalic acids. These substances were mixed together, allowed to stand for some time, then exposed to a high temperature, and finally saturated with carbonate of lime.

In some other experiments, oxalic acid in different quantities, one time in excess, another time sparingly, was mixed with glycolate of lime. In one experiment the mixture was allowed to stand for some days at the ordinary temperature, in another it was evaporated nearly to dryness. But all these experiments did not lead to the formation of glyoxylic acid. I obtained only unaltered oxalic and glycolic acids and water. Oxalic acid takes away all the lime from glyoxylate of lime. If the latter contained already oxalate of lime, such a result would be highly improbable. A strong solution of glyoxylate of ammonia generates carbonic acid on boiling, and becomes brown. Glycolate of ammonia, mixed with an equivalent quantity of oxalic acid or binoxalate of ammonia, may be boiled and heated till all the water is expelled, without undergoing the least change.

All these facts do not agree with the view that glyoxylic acid contains oxalic acid already formed.

The volatility of glyoxylic acid by the heat of the water-bath, and its apparent incapability of forming salts containing for one atom of metal more than two atoms of carbon, induce me to adopt for it the formula $C^2\ H^4\ O^4$, and not $C^4\ H^8\ O^8$.

From this and the composition of the lead-salt, and also from the behaviour of glyoxylate of lime towards lime, &c., it follows that glyoxylic acid is a bibasic acid, and that its formula must be written

$$\left.\begin{array}{l} C^2\,H^2\,O^2 \\ H\ \ H \end{array}\right\}O,$$
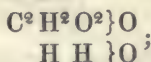
it is isomeric with formic acid, and contains twice as much oxygen as acetic acid.

Glyoxylate of lime on heating smells like burning sugar, and has in its external appearance a great resemblance to tartrate of lime, so much so that for some time I mistook it for the latter. The solid salt gives with nitrate of silver-ammonia a bright coating of silver on the sides of the glass tube, becomes electric at 100° C., and is only soluble in a large quantity of water; its crystals are hard. These qualities also belong more or less to tartrate of lime. The formulæ of the two acids, placed side by side,

$$\left.\begin{array}{l} C^2\,H^2\,O^2 \\ H\ \ H \end{array}\right\}O \quad \text{glyoxylic acid,}$$

$$\left.\begin{array}{l} C^4\,H^4\,O^4 \\ H\ \ H \end{array}\right\}O \quad \text{tartaric acid,}$$
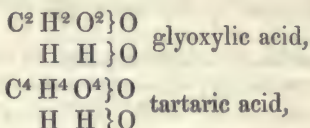
show that the two radicals $C^2\,H^2\,O^2$ and $C^4\,H^4\,O^4$ are isomeric, and that the formula of the one multiplied by two gives the formula of the other. There is also another connexion between both. Nitrotartaric acid, on decomposition by water, is converted into tartronic acid. The latter loses at a higher temperature water and carbonic acid, and becomes changed into glycolide, which on boiling with water is converted into glycolic acid. This acid plus one atom of oxygen leads to glyoxylic acid:—

$$\underbrace{C^4\,H^6\,O^6}_{\text{Tartaric acid.}} + O^2 - CO^2 - H^2O = \underbrace{C^3\,H^4\,O^5}_{\text{Tartronic acid.}}$$

$$\underbrace{C^3\,H^4\,O^5}_{\text{Tartronic acid.}} - CO^2 - H^2O = \underbrace{C^2\,H^2\,O^2}_{\text{Glycolide.}}$$

$$\underbrace{C^2\,H^2\,O^2}_{\text{Glycolide.}} + H^2O = \underbrace{C^2\,H^4\,O^3}_{\text{Glycolic acid.}}$$

$$\underbrace{C^2\,H^4\,O^3}_{\text{Glycolic acid.}} + O = \underbrace{C^2\,H^4\,O^4}_{\text{Glyoxylic acid.}}$$
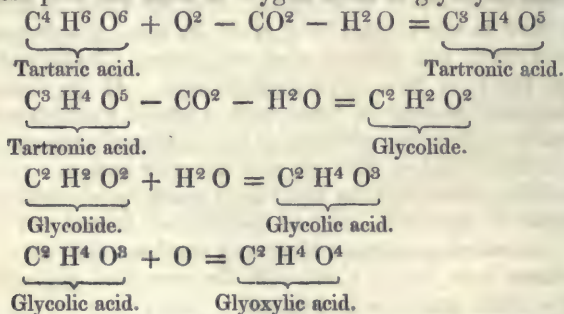
Since it is known that the properties of one alcohol are repeated generally in all the other alcohols, we may with great probability predict the existence of a series of acids of the general formula $C^n\,H^{2n}\,O^4$ or $C^n\,H^{2n}\,O^8$; and if all the members are

decomposed by alkaline substances in a similar manner as gly-
oxylic acid, a method for the production of all the acids homo-
logous to oxalic acid is given, viz.

$$2(C^3 H^6 O^4) + 3KHO = C^3 H^5 K O^3 + C^3 H^2 K^2 O^4 + 4H^2 O.$$

The results of the examination of the aldehydes, produced by
the action of nitric acid on alcohol along with the glyoxylic acid,
will be communicated in another paper.

Queenwood College, near Stockbridge, Hants.

XLV. *On a Solution of the Theory of Parallels, from the Defi-
nitions of Euclid, without deviating from the ordinary Principles
of Geometrical Proof. By* J. P. HENNESSY*.

IN my remarks on Professor Stevelly's paper, I ventured to
call attention to the fact that Euclid's definition of a square
bore an analogy to the definition of parallels in which an equality
of angles to a constant quantity is made the differentia. A con-
sideration of the origin and nature of this analogy has led me to
perceive, that in the former definition, as well as in the latter,
an assumption is made, which, when legitimately employed in
connexion with other data furnished by Euclid, leads to a strictly
geometrical solution of the doctrine of parallels.

It has often been remarked that Euclid's definition of a square
was partly based on what has been called an unnecessary assump-
tion. This so-called unnecessary assumption is said to consist
in the statement, that a four-sided equiangular and equilateral
figure *has its angles all right angles*; inasmuch as that this pro-
perty of the figure, instead of being implied in its definition,
might have been made the subject of subsequent demonstration.

One writer†, who aspired to make geometry a perfectly exact
science, seems to have been so impressed with this idea, that
he thought it necessary to remove the definition of a square from
its usual position among the premises of Euclid, and to place it
between the 34th and 35th propositions of the first book. But
whilst the assumption that Euclid has made has frequently been
noticed, its legitimate consequences have not attracted any atten-
tion. These consequences appear, however, to be of no small
importance.

The various attempts to solve the theory of parallels may be
divided into three classes. (1) Those which essentially consisted

* Communicated by the Author.
† Colonel Perronet Thompson.

in the introduction of a method of reasoning different from that employed by Euclid. This is an extensive class. It includes the attempts of Professor Franceschini\*, of M. Legendre, of MM. Bertrand and Lacroix†, of Dr. Oliver‡, of Mr. Playfair§, of Colonel Thompson ||, and of many other writers. (2) Those which, preserving Euclid's method of reasoning, were based on a change in his definitions. To this class belong the attempts of many ancient geometers, as well as of many modern English geometers. (3) Lastly, those attempts which were made to solve the difficulty by taking Euclid's definitions as they stand, excluding the 12th axiom, and employing the ordinary method of geometrical demonstration.

Of the two first of these classes it is unnecessary for me to say anything. Of the third class it is only necessary to observe, that no successful attempt has hitherto been published.

Those who believe that the foundation of a system of geometry should be laid as narrow as possible, and that nothing should be taken for granted in the commencement which might possibly be made the subject of subsequent proof, will probably assert that Euclid's definition of a square is defective, and should be altered. I allude to this for the purpose of observing that such a question is foreign to this paper. It is not with the propriety or impropriety of Euclid's definition that I am now dealing. I am dealing simply with that definition as we find it, and with the assumption on which its possibility is based.

That assumption, as stated in formal language, is as follows :— "A quadrilateral, the sides of which are respectively equal to each other, and the angles of which are also respectively equal to each other, contains four, and only four right angles."

This assumption of Euclid's can be employed in either of two ways : either in the shape of a problem, on the well-known principle that the theorems of Euclid may be proved independently of the problems, the construction of which can be assumed ; or, which seems to be the more satisfactory course, by using it as one of the regular premises of a theorem. It is in the latter way that it is here employed.

## PROPOSITION I.

*The three interior angles of a right-angled isosceles triangle are together equal to two right angles.*

Produce the sides AB and AC, which include the right angle,

---

\* *La Teoria delle parallele rigorosamente dimostrata : Opuscoli Matematichi,* 1787.

† *Elémens de Géométrie,* p. 23.

‡ *De rectarum linearum parallelismo, &c.* 1604.

§ Geometry, p. 409.         || Geometry without Axioms, p. 84–98.

until the produced parts are equal to AB and AC. Draw DB, DE, and EC. The angles BAC and BAD are equal to two right angles (XIII.)*, and are therefore equal to each other; the angles DAE and CAE are also equal to two right angles; therefore the four angles at A are right angles. In the triangles BAC and BAD, two sides of the one (BA and AC) are equal to two sides of the other (BA and AD), and the included angles are equal; hence BC is equal to BD, and the angles ABC and ACB are equal to ABD and ADB. In the same way the sides DE and EC can be shown to be equal to each other and to DB and BC; and the angles in each triangle to be respectively equal. Hence (from the definition of a square) these four angles are right angles; and ABC and ACB are half right angles. Therefore the right-angled isosceles triangle ABC contains two right angles.
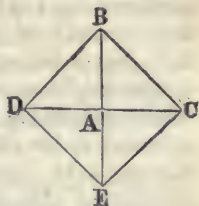
LEMMA I. *If one of the angles* (ABC) *at the hypothenuse of a right-angled triangle be equal to half a right angle, the triangle is isosceles.*

If AB and AC are not equal, let AC be made equal to AB; and draw BD.

As BAD is an isosceles right-angled triangle, the angle ABD is equal to half a right angle. But ABC is equal to half a right angle; therefore a part is equal to the whole, which is absurd; therefore AC must be equal to AB.
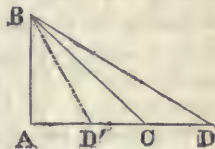
LEMMA II. *If, in two triangles* (ABC *and* DEF), *two sides of the one* (AB *and* BC) *be respectively equal to two sides of the other* (DE *and* EF), *and an angle* (BAC), *greater than a right angle, in the one be equal to an angle* (EDF) *in the other, the two triangles are equal in every respect.*

Let the triangles be applied to each other so that the bases
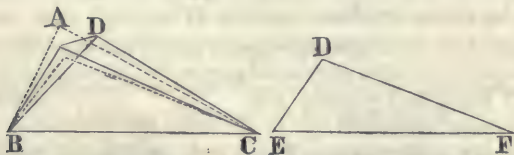
BC and EF coincide. Then DE will lie on AB. If not, let DEF take the position BDC. Join A and D.

As AB and BD are equal, the angles DAB and ADB are

equal. Therefore BDA is greater than a right angle. Therefore
in the triangle ADC there is one angle greater than two right
angles, which is impossible (XVII.). Nor can the triangle EDC
lie above or below AB and AC (as the dotted lines in the figure),
for the angles at A and D are equal (XXI.) ; therefore ED must
fall on AB, and therefore the triangles are, in every respect, equal.

## Proposition II.

*The three interior angles of every right-angled triangle are together
equal to two right angles.*

Let ABC be a right-angled tri-
angle; produce AB till AD is equal
to AC, and take, in AC, AE equal to
AB.    Draw DC and BE.    At the
point B draw BF perpendicular to AD.
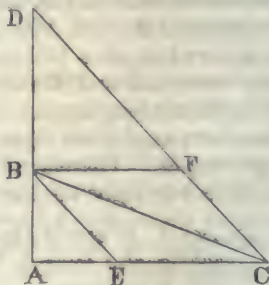
As the triangle DAC is an isosceles
right-angled triangle, the angles at
D and C are equal to half right
angles (Prop. I.).    The triangle BDF
is isosceles, because one angle, DBF,
is a right angle, and another, BDF,
is half a right angle (Lemma I.).    The angle CFB is greater
than a right angle, and, being the supplement of half a right
angle, is equal to CEB, for the angle AEB is equal to half a
right angle (Prop. I.).    BF is equal to BD ; BD is equal to EC ;
therefore BF and EC are equal.

In the triangles BFC and CEB, two sides, and an angle greater
than a right angle, are respectively equal ; therefore the triangles
are equal in every respect (Lemma II.) ; therefore the angles
EBC and ECB are equal to the angles FCB and FBC.    Now
the angles FBE and FCE are each half right angles ; therefore
the angles EBC and ECB are together equal to half a right
angle.    To these add ABE, which is half a right angle, and
BAE, which is a right angle ; then the three interior angles of the
right-angled triangle ABC are together equal to two right angles.

## Proposition III.

*The three interior angles of every triangle are together equal to
two right angles.*

Let fall upon the greater side
AC the perpendicular BD.

In each of the right-angled tri-
angles thus formed there are two
right angles (Prop. II.).    Taking
away the two right angles at D,
there remains the three angles of
the triangle ABC, together equal to two right angles.

It was in the attempt to establish this theorem that M. Legendre deviated from the method of Euclid by employing the doctrine of limits. Setting out from this theorem, the demonstrations of the various properties of parallels are simple and obvious. To give these final propositions would be only to repeat what M. Legendre has done; for to that distinguished geometer must belong the merit of having shown that the whole question depended upon a proof of the 32nd proposition. His proof of that proposition is inadmissible; but his subsequent demonstrations are in accordance with the principles of plane geometry.

Although the proof just given of this theorem will probably be acknowledged to be consistent both with the definitions and with the method of Euclid, nevertheless I cannot regard it as the most satisfactory solution of the difficulty. It may be interesting as the first successful attempt of the class to which it belongs. But the real solution I still believe to consist in treating the conception of parallel lines, as we treat any other geometrical conception, with a clear and unequivocal definition.

119 Jermyn Street,
    October 8, 1856.

---

### XLVI. *On the Doctrine of Parallel Lines.*
### *By Professor* STEVELLY.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN, Holywood, Oct. 13, 1856.

FOR the personally kind terms in which Mr. Hennessy has stated his objections to the method of treating the doctrine of parallel lines which I lately communicated to you, I beg to express my acknowledgements. I also feel much gratified that he does not profess to have detected any defect in the chain of reasoning on which the proofs are founded.

I trust, however, Mr. Hennessy will not consider me deficient in courtesy when I add, that regard for scientific truth compels me to say, that there is scarcely an assertion which he has made in that paper, or in the one to which he refers in (the fifth not) the third volume of the present series of this Journal, in which I can concur.

Since a detailed examination of the defective logic and incorrect statements contained in those papers would far exceed any reasonable limits, and since an examination of even two or three of the most obvious would wear too much the appearance of controversy, and seem to invest the entire subject with a very undue share of importance, I shall simply select one as an example, because of its close connexion with the subject of my communi-

cation, and because it affords a fair specimen of the defective logic and incorrect reasoning which pervade the whole.

To sustain his position, that the method of limits resorted to by me, if allowable, was unnecessary, Mr. Hennessy asserts that he has shown in the third (fifth?) volume of the present series of this Journal, that the entire doctrine of parallel lines may be derived from the following definition :—" Parallel lines are such, that if they meet a third line, the two interior angles on the same side will be (together) equal to two right angles."

Now it is quite certain that this definition, as such, does not give a solid foundation for the doctrine of parallel lines, neither does it warrant the deductions derived from it in the paper referred to. Let us examine it even partially.

I meet two lines, *wx* and *yz*, so related to "a third" line, *wy*, that the two interior angles on the same side are together equal to two right angles. Well, the definition fairly warrants me in calling
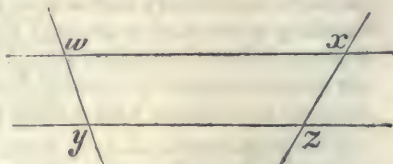


these lines "parallel." This is the essential difference which distinguishes them from other pairs of lines, and from which all their properties must be proved to flow. Now the 29th proposition of Euclid's Elements calls on me to prove that "any" line, as *xy*, which cuts those, also makes the two interior angles on the same side together equal to two right angles. This is a relation of these two lines to all others which cut them which requires to be proved, and which neither the above definition nor any other definition can warrant us to assume as a truth without proof. It is a "property" which must be shown to be an essential concomitant of that which has already fixed the relation of those two lines to one another, and to all others which cut them, and which we have by the definition merely agreed shall entitle us to denominate them "parallel lines" in any individual case in which we find it to obtain. And yet this simple assumption is all that Mr. Hennessy gives in the paper referred to as proof of the 29th and 27th of Euclid's first book of Elements.

When I was a boy at school, I thought for a day or two that I had proved the entire doctrine of parallel lines by defining them to be such, that if "any line" was drawn across them they would make the alternate angles equal. But I am happy to add that I detected my false logic before I had committed myself by its publication.

I am, Gentlemen,
Your obedient Servant,
JOHN STEVELLY.

XLVII. *On a powerful Form of the Induction Coil, with some new Statical and Thermal Effects of the Induced Current.* By JONATHAN N. HEARDER, *Plymouth\*.*

ON reading in the Philosophical Magazine for January 1855, an account of the very interesting experiments performed by Prof. Grove with a powerful induction coil constructed by M. Ruhmkorff of Paris, I immediately made inquiries respecting the instrument, but learnt that its construction was such, every part being thoroughly imbedded in melted shell-lac, that nothing was known of its internal arrangement. Having often promised myself to make an instrument of the same kind, I proceeded at once to act upon known principles in the construction of an induction coil, in which the secondary wire should be, as far as practicable, perfectly insulated, taking care at the same time to combine with it the facility of removing and relaying it to correct errors or repair injuries.

My first attempt was eminently successful, since I produced effects far exceeding those of M. Ruhmkorff's, with about one-third of the quantity of secondary wire reported to be contained in his machine. In March of the present year, I delivered a paper on the subject to the members of the Plymouth Institution and Devon and Cornwall Natural History Society, and then first exhibited the machine in public. Subsequently in the same month it was also exhibited before the members of the Plymouth Mechanics' Institute by an eminent electrician of London, who, when lecturing on the subject of the induction coil, availed himself of the opportunity of comparing it with one of M. Ruhmkorff's machines, and who at once gave his decision in favour of my arrangement. I should have published a description of it at the time, but having furnished him with minute particulars of the construction of my apparatus, together with my views respecting the scientific principles involved in its action, for the purpose of insertion in a new work on electricity which he had then almost ready for publication, I promised not to do so until his work was out. It has not, however, yet appeared; and I have now learnt that the same gentleman has recently delivered a lecture on the induction coil at one of the scientific institutions of the metropolis, in which he brought forward a machine professing to be an improvement on M. Ruhmkorff's apparatus; and as it is almost identical in construction and arrangement with mine, I have felt some little surprise at his not having made the slightest allusion to the fact of my having constructed one prior to the one which he had brought forward, particularly since at his own especial request I had refrained from publishing any particulars of my own arrangement. In the course of an extended series

of investigations with my instrument I had occasion to relay the secondary wire, and in doing so I only put on five-sixths of the original quantity, but I have had the satisfaction to find the statical power of the machine nearly doubled. I exhibited this new arrangement at the Royal Cornwall Polytechnic Institution on the 17th of September last, and was honoured by the Society's first silver medal. The main object of the present paper is to detail some of the results of my investigations with it on the character of the induced current, as I have discovered some facts which, as far as I know, are quite new, and which seem to develope a new law of electrical action.

2. Hitherto, I believe, the only thermal effects observed in metallic conductors by the action of the induced current of the induction coil, have been the incandescence of the end of the negative terminal, which, when made of very fine wire, melts and burns, and the partial combustion of portions of metallic filings when the spark passes between them as they lie strewed upon a non-conducting surface, as is evidenced by the colour of the spark varying with the nature of the metal used. Whilst the spark in its passage through inflammable bad conductors, such as paper, cotton, gunpowder, and even through inflammable non-conductors, such as æther, alcohol, turpentine, &c., readily kindles them, yet the same current made to pass through an extremely fine wire in a closed circuit does not sensibly heat it. In this particular the spark of the induction coil resembles that of a discharge from a Leyden jar when made to pass through an imperfectly conducting circuit, such as a wet string, or tube of water; for it is to be observed, that although a charge from a Leyden jar or battery, when passed at once through a fine wire, may be sufficient to melt and dissipate it, yet it will not sensibly heat the same wire when water is in any way made part of the circuit, though the same spark will in the latter case inflame gunpowder, whilst through the perfectly conducting circuit it disperses it without ignition. There can be very little doubt that the discharge requires a certain amount of time to pass through the imperfect conductor, and that the metal is thus able freely to transmit the electricity with its reduced velocity, and is consequently not heated by it. Effects analogous to these are produced by the induction coil; for although the soft static spark between the terminals will inflame gunpowder, yet the discharge from a Leyden jar, when its coatings are in connexion respectively with each terminal, merely disperses it, as when charged with frictional electricity.

3. I had often felt that if any definite thermal results could be obtained from the action of the Leyden jar in connexion with the induction apparatus, a channel would be opened up through which an accurate comparison might be made between the cha-

racter of frictional and voltaic electricity, and I believe that I have now succeeded in indicating a path through which this important object may be obtained. In order to examine the thermal effects of the Leyden jar, I had recourse to a modification of Sir W. Snow Harris's thermo-electrometer, which I introduced about twenty-six years since, in order to adapt the instrument for voltaic as well as frictional electricity, and for which, in 1844, I received the first bronze medal of the Devonport and Stonehouse Polytechnic Exhibition.

4. My apparatus differs from that of Sir W. Snow Harris only in the construction and arrangement of the air-vessel which contains the wire. In Sir W. Snow Harris's instrument (Phil. Mag. No. 73. May 1856, p. 343, fig. 5), the wire passes horizontally and diametrically through a glass globe, having external brass caps on the ends of the wire for the purpose of making connexions. The air-chamber which I employ is a cylinder about $3\frac{1}{2}$ inches diameter, with two necks like a small electrical cylinder placed vertically. The necks are capped with brass; the lower one screws upon the reservoir of the bent indicating tube, and the upper one is furnished with a stuffing-box, through which passes vertically a sliding wire surmounted with a binding screw, and formed at the lower and inner extremity into a forceps. The test wire hangs vertically in the axis of the cylinder, the upper end being attached to the sliding forceps, whilst from the lower end hangs a short detached forceps, the tail of which can be lowered into a brass cup containing mercury. This cup is in connexion with the brass screw of the reservoir upon which the cylinder is screwed, and to which is also attached a second binding screw for completing the circuit. There is also an adjusting valve in the cap of the reservoir. The advantages of this arrangement are, that it affords the greatest facility for changing the wire, the stuffing-box being made to unscrew from the cap of the cylinder, whereby the wire with its lower forceps can be lifted out of the mercury and other wire substituted, or the length of the same wire varied in a few moments. The instrument also affords facilities for examining the effects of the voltaic arc, &c. The principle, however, is the same as that originally contrived by Sir W. Snow Harris, who has the merit of having first applied it as a measure of electrical forces; and with the exception of the air-chamber and its appliances just described, is in all other respects similar in construction to his.

5. I was anxious to know whether the discharge from the Leyden jar, when charged by the induced current, possessed the same character as that obtained from the charge of an electrical machine; for I could scarcely bring myself to believe, that, in the rapid succession of discharges which I could procure from my coil, amounting sometimes to more than 200 per second

through an interval of 0·5 of an inch, the jar was really as fully charged as when charged in the ordinary way. I therefore instituted a set of experiments which have developed some curious and interesting results, and which, as far as I know at present, are quite new.

6. I introduced between the secondary terminals of the coil a thermo electrometer in connexion with a Lane's discharger, but although I varied the length of the sparks from 0 to 1 inch, no heating effect was produced upon the wire in the instrument.

7. I connected the coatings of an insulated Leyden jar, containing about $3\frac{3}{4}$ square feet of surface, respectively with the two terminals of the coil, and completed the circuit of the jar by a thermo-electrometer in connexion with a graduated Lane's discharger. On causing a rapid series of discharges to pass, the fluid instantly rose in the stem and remained fixed pretty permanently at nearly 100 degrees, or about one hundred times higher than it would have risen with a single discharge from the same jar with the ordinary electrical machine. The platina wire hanging in the cylinder of the electrometer was thrown into the most energetic vibration during the passage of the sparks, so as to become crippled in several places.

8. The thermo-electrometer was removed, and a piece of very fine iron wire, about 4 inches long, placed between insulated forceps, was substituted for it in the circuit. On passing the discharges, the wire became blue, vibrated rapidly, and crippled in several places; and whilst occasionally suspending and reproducing the action of the coil, the wire suddenly melted at one of the crippled places.

9. Another piece of the same wire, about 3 inches in length, was next introduced, and on passing the discharges it instantly became red-hot through nearly its whole length and fused into globules. The experiment was repeated with shorter pieces with nearly the same results, the wire sometimes fusing throughout its whole length, and sometimes breaking into fragments partially burnt.

10. The foregoing remarkable change in the thermal character of the current when intercepted and, as it were, measured out by the jar, induced the following experiment :—The thermo-electrometer was introduced into the circuit of the jar as before, and two other thermo-electrometers were introduced into the portions of the circuit leading from the terminals of the coil to the coatings of the jar. By this arrangement, the current which charged the jar had to pass through these electrometers in its course. On setting the machine in action, the electrometer which completed the circuit of the jar indicated the same thermal effect upon the included wire, whilst the other two electrometers were not in the slightest degree affected, clearly showing a marked difference

in the velocity of the current under the two different con-
ditions.

11. In the foregoing experiments, the discharging electrometer
was fitted with platina points, between which discharges can be
obtained of a greater length than between balls; but in order to
place the experiment under conditions which would admit of
better comparison with the effects of the ordinary electrical
machine, I substituted balls for points in the discharger. On
placing them at such a distance from each other as to allow the
discharges to pass, the fluid instantly rose to a definite height in
the tube, nearly 100 degrees; and whilst thus permanent, the
balls of the discharger were gradually approximated to each other,
with no diminution of effect, until they were in absolute contact,
when, the circuit being closed and the jar no longer capable of
receiving a charge, the fluid instantly fell nearly to zero, the
residual amount of expansion being due to the heat absorbed by
the glass surface, &c. during the continuation of the experiment.
On the slightest separation of the balls from each other the fluid
rose to 100 degrees, and remained tolerably permanent until
they were separated beyond the striking interval, when the fluid
again fell as soon as the discharges ceased.

12. The balls were next placed rather within the greatest
striking distance; and instead of allowing the spring to vibrate
and produce the ordinary rapid succession of discharges, I held
it in my hand and broke contact at successive single slow inter-
vals, in order to ascertain the value of a single discharge; this
I found to be only 1 degree on the scale of the instrument. On
making an interruption of the circuit, the action of the fluid was
precisely of the same character as if the jar had been charged by
the electrical machine, that is to say, the fluid rose with a sudden
impulse and instantly subsided. On repeating the interruptions
at rather quicker intervals, so as to make each successive inter-
ruption before the effect of the preceding one had quite subsided,
the fluid was made to rise 1 degree in succession for each dis-
charge, so that 1, 2, 3, and so on to 12 discharges, just raised
the fluid correspondingly to 1, 2, 3, to 12 degrees. There was
some little difficulty in manipulating in this way, as the discharge
did not always take place at each interruption in the circuit,
owing to variations in the character of the metallic contact at
the interruptor, arising from the oxidation of the platina surfaces;
but as far as they went they were sufficient to show that the extra-
ordinary result first noticed was due to a rapid succession of
units of effect gradually accumulating in the transmitting wire.

13. Having thus established a means of comparing the effects
of the coil with those of the ordinary electrical machine, and
having ascertained the value of a unit of discharge under the
influence of the induction coil, it next became necessary to com-

pare this with a similar unit when charged by the electrical machine; and this I found, with the striking distance which I had employed, as nearly the same as so small an indication on the scale as 1 degree would enable me to judge. On increasing or diminishing the striking distance of the balls when using the electrical machine, the fluid indicated the action of the well-established law, that the effect is in proportion to the square of the striking distance, the charged surface being constant. No such law as this was however observable with the induced current; for, on the contrary, it was ascertained by subsequent experiments that the effect on the wire was diminished when the striking distance was prolonged beyond a certain limit, a fact at variance with hitherto-recognized laws of electrical action.

As the foregoing experiments were performed with a large jar, which required that the induction coil should be very intensely excited, thereby rendering its action less permanent, I subsequently employed smaller jars, and found the same class of results to accompany their action, but with others which the conditions of the first experiments had not enabled me to discover.

14. Having satisfied myself that the nature of the charge in the jar was the same with the induction coil as with the machine, I next proceeded to examine the effect of varying the extent of surface; and as my object was more to compare results with each other, under the action of the induced current, than to establish any connexion between these phænomena and those of the common electrical machine, I resumed the use of points in the discharger, which, from the greater length of spark obtainable by them, enabled me to vary the striking distance over a greater extent, and thereby more closely to examine the differences of effect.

15. A jar containing 3 square feet of surface was employed, and the thermo-electrometer gave an indication of about 90 degrees, falling 3 or 4 degrees as the points were withdrawn to the greatest striking distance nearly 0·3 of an inch, but rising again as the distance lessened, until they were brought so near that the negative point became heated, at which time the fluid fell 20 or 30 degrees, the diminution of temperature in the electrometer being greater as the negative point became hotter. As soon as the negative terminal became heated, the noise of the sparks was much less, and they assumed a more diffused and flame-like character. On withdrawing the points again from each other, the temperature in the electrometer increased as the heat of the negative terminal diminished; and when the points had been so far withdrawn that the negative terminal ceased to be heated, the sparks suddenly resumed their noisy character, and the fluid as suddenly rose to about 90 degrees.

16. A jar of 1 square foot, but of rather thinner glass, pro-

duced a maximum temperature of 35 degrees, but the sparks between the terminals were much longer, averaging about 0·4 of an inch. The temperature fell a few degrees on pushing the striking distance to its greatest extent, but was pretty constant at all distances between the extreme length and the shorter interval, about 0·1, at which the negative point became ignited.

17. A jar of half a square foot, of rather stouter glass, about the same thickness as the large jar, gave a maximum temperature of 15 degrees, and the maximum length of spark was 0·5 of an inch; but the negative terminal now became heated, when the striking distance was reduced to 0·2 of an inch; but between these extremes the temperature indicated by the electrometer was not influenced by any variation in the length of the sparks.

18. On comparing the results of these jars with each other, they appear to have produced effects bearing a direct relation to the extent of their coated surfaces; for although the jar of 1 square foot gave an effect rather more than was due to its relative surface, yet being thinner it was capable of holding rather a stronger charge; and without the least desire to coax results, the simple relation of effects to causes appears here strikingly manifest.

19. In a subsequent series of experiments an extra unit of measure, containing a quarter of a square foot, was added; and here the effects were in relation to each other as 1, 2, 4 + 12, corresponding to the relative extent of the surfaces of the jars, the thin jar of 1 square foot (4 +) still remaining the exception. When however two jars of 3 square feet were connected as a battery, so as to double the surface of the last element, the resulting temperature was only 115 degrees, and the spark between the terminals much shorter.

20. In each set of experiments care was taken to conduct the whole as speedily as was consistent with accuracy, so as to obtain the whole of the results before the action of the voltaic battery began to subside; and it was curious to remark, that as it gradually lost power, so the largest jar gradually lost the relationship which it originally bore to the others.

21. The results here detailed appear to indicate, that, with a given surface, the heating effect of the electrical discharges does not vary with the length of the spark within given limits, other things remaining the same.

22. That the heating effect of different surfaces appears to be in the direct simple ratio of their respective areas, up to the point at which the quantity of electricity transmitted by each wave of the induced current is capable of developing the maximum effect of the coated surface in relation to its own tension.

23. On varying these experiments by exciting the coil more or less powerfully, the same relation between surface and heating effect was observed whatever was the value of the unit indicated

by the electrometer, excepting that as the power of the instrument was greater or less, so the extreme indications of the largest surface correspondingly increased or diminished.

24. It is well known that the soft static spark between the terminals of the coil inflames gunpowder, whilst the explosive discharges when the jar is connected only disperse it without inflammation. Hence it would appear that there is a difference in the velocity with which the wave or current is transmitted; for although the quantity must be very considerable, yet its velocity appears to a certain extent retarded in its passage through the induced wire, and that the effect of the jar appears to be to measure out certain quantities, which are transmitted with greater energy and velocity through that portion of the circuit forming the connexion between its own inner and outer coatings, than through those portions by which these coatings are supplied, and hence the difference in the thermal effects of these different portions of the circuit upon metallic wires and gunpowder.

25. In order to test the correctness of the foregoing opinion, a small jar of half a square foot was connected in the ordinary manner with the coil (par. 7), and a thermo-electrometer placed in the circuit of the jar. A portion of gunpowder was placed between the points of the discharger. On passing the discharges, the gunpowder was dispersed, but the electrometer indicated the usual rise of temperature. On now inserting a wet string as a portion of the circuit of the jar, the discharges lost their noisy character and inflamed the gunpowder, but the thermo-electrometer remained perfectly unaffected.

26. Whatever be the character of this secondary wave, or of the sources from whence it is derived, it is clear that an amount of electrical disturbance is produced far exceeding what we have any ordinary notions of; and when compared with the results obtainable from the most powerful apparatus for the development of frictional electricity, the effects of the latter appear to dwindle into insignificance. I cannot help feeling, however, that, by having developed a class of phænomena which are capable of being produced both through the agency of voltaic and frictional electricity, I have established another link of connexion between the two agencies, and one which may ultimately serve to determine the nature of the relationship existing between them. In this investigation I am at present engaged, and hope to make further developments the subject of a future communication.

27. In concluding the present crude paper, I may remark that on first witnessing the torrent of discharges from a large Leyden jar, I immediately recognized in them an exact correspondence with certain phænomena which I had before observed in experimenting with an atmospheric exploring wire during thunder-storms, and of the real character of which I had scarcely

been able to convince myself. This atmospheric wire was about 400 feet in length, suspended between the top of my own house and the turret of a distant chapel, and insulated at the two extremities by silk strings which were under cover. A second insulated wire proceeded from the first into my laboratory, and was there in connexion with a battery of three Leyden jars, containing together about 10 square feet of surface; these were connected with a discharging electrometer, terminated by balls, and which was capable of being adjusted by silk strings and cranks, carried like bell-wires to a place of safety. During the continuance of a thunder-storm, I have frequently noticed the extraordinary inductive action produced upon the wire at the instant of a flash of lightning, though at a distance of two, and even three miles, as evidenced by the number of seconds elapsing before hearing the thunder. This action was frequently so great as to cause an instantaneous torrent of discharges from this battery, over an interval of three-tenths of an inch, of precisely the same character as those produced by the induction coil, but very much louder. Never having employed any test of the quantity thus discharged, I could scarcely imagine that the jars were really charged and discharged at so rapid a rate from so small a wire; but the experiments with the coil now perfectly satisfy me as to the identity of the character of these atmospheric discharges, and I merely allude to them as the only ones approximating in effect to those developed by the induction coil that I have ever witnessed.

XLVIII. *Note on* Prof. Clausius's *Application of the Mechanical Theory of Heat to the Steam-engine. By* J. P. Joule, *Esq.**

A S the motives which induced Professor Thomson and myself to undertake experiments on the thermal effects of fluids in motion appear to me to be somewhat misunderstood by Professor R. Clausius in his paper published in the October Number of this Magazine, I may be allowed to refer to Prof. Thomson's paper "On the Dynamical Theory of Heat" (quoted by Clausius), of which Part IV. is devoted to the discussion of a method of discovering experimentally the relation between the mechanical work spent and the heat produced by the compression of a gaseous fluid. In it, referring to the hypothesis first assumed by Mayer, that the work spent in compressing a gas is exactly equivalent to the heat evolved, he notices my paper "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," in which the hypothesis is verified as far as the limits of experimental accuracy permitted. Prof. Thomson then proceeds, § 72–77†, to point out a method whereby an excessively delicate test of Mayer's hypothesis may be obtained for any

* Communicated by the Author. † Phil. Mag. vol. iv. p. 429.

temperature,—the method, in fact, which we have employed in our experiments referred to by the celebrated German physicist. At the commencement of the memoir* containing the first results of these experiments, we remark that Mayer's hypothesis, which has been adopted by Holtzmann, Clausius, and other philosophers, had been verified approximately for air at ordinary temperatures by my experiments, and by Prof. Thomson's theoretical investigation, founded on a conclusion of Carnot's which requires no modification in the dynamical theory of heat.    I may add, that in Prof. Thomson's account of Carnot's theory, communicated to the Royal Society of Edinburgh, April 1849, the formula is demonstrated which gives Carnot's function in terms of the temperature, and the ratio of the work spent to the heat evolved in compressing air kept at the same temperature.    The results we have arrived at in our joint investigation have tended certainly to develope our views, and to give a more definite knowledge of the constitution of elastic fluids, but they do not contradict our original statements, published before the appearance of Prof. Clausius's papers.

Oakfield, Moss Side,
Oct. 20, 1856.

---

### XLIX. *Note on the " Davidsonite" of* Thomson.
#### *By* Dr. Heddle†.

SOME time ago I obtained from the Rev. Dr. Fleming a portion of a crystal of " Davidsonite," which he requested me to analyse.    The Doctor informed me that he had obtained from it *glucina*, and believed it to be merely beryl, and not a distinct species, a conclusion at which I had myself arrived from a simple mineralogical examination of the substance.    The crystal on analysis afforded 12·52 per cent. of glucina.    I do not give the full analysis, as the specimen was contaminated with plates of mica and grains of quartz, which I was unable entirely to exclude ; from this cause the proportion of glucina must be somewhat greater than that stated.    Setting aside the above mechanical impurities, the analysis in all respects agreed with that of beryl.

Thomson considered the substance to be oblique, as his crystal had an oblique termination ; this was the result of one of the terminal planes truncating so deeply as to carry off all the others. All the crystals are rough and coarse.

The localities at Aberdeen are Tory, opposite the mouth of the harbour, and the Rubieslaw quarry ; at both it occurs in coarsegrained granitic veins, which traverse the ordinary compact granite of the district.    At Rubieslaw the crystals of felspar in these veins are twins of very large dimensions.

* Phil. Mag. vol. iv. p. 481.
† Communicated by the Author.

## L. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 316.]

Jan. 31, 1856.—The Lord Wrottesley, President, in the Chair.

THE following communication was read :—

"A Description of a new Sphygmoscope, an Instrument for indicating the Movements of the Heart and Blood-vessels ; with an Account of Observations obtained by the aid of that Instrument." By S. Scott Alison, M.D.

The sphygmoscope (fig. 1) consists of a small chamber containing spirits of wine or other liquid, provided with a thin india-rubber wall, where it is to be applied to the chest. At the opposite extremity the chamber communicates with a glass tube, which rises to some height above the level of the chamber. Liquid is supplied to the instrument until it stands in the tube a little above the level of the chamber. The pressure of the column of liquid in the tube acts upon the elastic or yielding wall of india-rubber and causes it to protrude. This protruding part or chest-piece is very readily affected by external impulse ; it yields to the slightest touch, and being pushed inwards, causes a displacement of the liquid in the non-elastic chamber, and forces a portion of liquid up the tube. The protruding wall of india-rubber is driven inwards when it is brought in contact with that portion of the chest which is struck by the apex of the heart, and a rise in the tube takes place. When the heart retires, the india-rubber wall, affected by the pressure of the column of

Fig. 1.
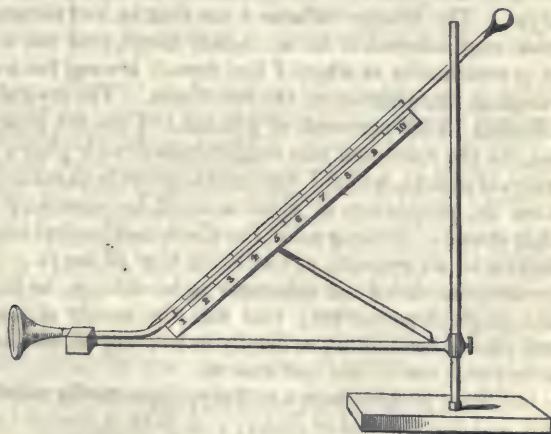


Sphygmoscope.

liquid in the tube, is pressed back, follows the chest, and permits the liquid to descend. The degree to which the india-rubber wall is

forced in by the apex of the heart is denoted by a corresponding rise in the tube, and the amount of protrusion of the india-rubber wall which takes place when the heart retires is denoted by a corresponding fall in the tube. The tube is supplied with a graduated scale to denote the rise and fall with exactitude. The glass tube is provided at the top with some contrivance, such as a brass screw and collar, to prevent the egress of the liquid when the instrument is not in use, or a bulb with an orifice may be supplied. When employed, the glass tube is left open to permit of the passage of the air to and fro.

The sphygmoscope is mounted upon a stand. The chamber and tube are fitted to a horizontal arm, which is made to move up and down so as to carry the instrument to the desired height. The base is so made as to secure the requisite immobility.

The glass tube is a foot or more long, and the round bore is about the one-eighth part of an inch. If the bore be much larger, the movement will be inconsiderable ; if much less, capillary attraction will interfere and prevent free motion.

When the instrument is to be employed, mounted upon its stand, it is placed upon a firm table with the chamber projecting beyond it. The person whose heart is to be examined is seated upon a firm chair, with his chest erect and free from motion. The protruding india-rubber wall of the chamber or chest-piece is delicately made to touch the fifth intercostal space so as to receive the blow of the apex of the heart. The liquid in the tube is now observed to be in motion. With persons in ordinary health, the liquid rises and falls about an inch. This rise and fall, after taking place three or four times, is followed by a much longer rise and fall to the extent of three or four inches, due to the advancement and retirement of the wall of the chest during the acts of respiration. The shorter rise and fall are again repeated and are again followed by the longer rise and fall caused by the motions of the chest. During the longer rise and fall due to respiration, the beat and retreat of the heart are still to be recognized by brief interruptions in the rise and fall of the liquid. When difficulty is experienced in obtaining the shock of the heart sufficiently strong to give an appreciable rise and fall, the examinee should make a moderate expiration, and then hold his breath and incline the chest somewhat forward. When the action of the heart is feebly felt at the præcordial region, it may be necessary to apply the instrument to the naked chest; but this is not necessary in the great majority of cases, and it will generally suffice to make the shirt and waistcoat fit tight to the skin. In many trials the sphygmoscope has succeeded in indicating the movements of the heart through the tightly buttoned coat. Thin persons are very favourable for examination ; on the other hand, the corpulent less readily affect the instrument.

The movements of the heart, though best indicated at the fifth intercostal space, are to be denoted at other parts of the chest, and in some examples of disease and of large and powerful heart, even in the epigastric region. The moving arm proves convenient in apply-

ing the instrument to these parts. In many persons with no very excited heart, it is sensibly acted on at the scapular and infra-dorsal regions.

By means of this instrument the observer can ascertain the frequency of the beats of the heart, but as this can be effected in most cases with accuracy at the radial artery, no particular advantage is gained from it in respect to this point.

The duration of the impulse of the heart upon the chest is well measured by this instrument: the time occupied by the rise, is the time occupied by the impulse. A slow rise after a rapid rise shows a slow beat after a rapid one, and *vice versâ,* a slow fall after an ordinary fall, shows a slow retirement after an ordinary one. An intermittent pulse is marked by an imperfect rise, followed by an ordinary fall, and then by a long rise. The rise is sometimes slower than the fall, which is occasionally found to be abrupt. This is observed when the heart, by reason of its great size, and of the somewhat bent back posture of the thorax, suddenly falls away from the walls of the chest.

The movements of the auricles under ordinary circumstances are not indicated by the sphygmoscope, though when it is placed over them, the liquid in the tube is moved upwards and downwards; but as these movements are synchronous with the movements upwards and downwards of another instrument placed at the apex, it may be inferred that the ventricle is the cause of them all. In some examples of greatly excited heart, as in phthisis, the instrument has revealed movements which seemed to proceed from the auricles; but further observations are required to settle this point, as well as the question whether the movements of the aorta, in a state of excitement, communicate any influence to the instrument.

The instrument, placed upon the heart, indicates strokes of that organ which are so feeble as to have no corresponding pulse at the wrist.

No pause whatever in the movement of the liquid has been at any time observed when the sphygmoscope has been carefully placed so as to receive the full beat, and fall back with freedom. This would go to show that the heart, however slow, is in constant motion, and, contrary to the belief of many physiologists, enjoys no pause. There is certainly no pause in the descent of the liquid, which takes place when the heart retires from the thoracic walls, in the middle of which movement it has been said a very short pause is to be observed in living animals having the heart exposed.

The force with which the heart beats at the fifth intercostal space may be ascertained by closing the upper extremity of the glass tube, and observing the extent to which the enclosed air is compressed.

When the heart is excited, the liquid in the sphygmoscope rises and falls more than usual; but the rise and fall of the excited enlarged heart is much the same as the rise and fall of the excited normal organ. For the most part the enlarged heart gives movements to the instrument when placed upon the ribs and sternum, whilst the

normally sized heart affects more exclusively when it is placed upon the fifth intercostal space.

The sphygmoscope indicates with exactitude both the absolute and the comparative influence upon the heart, of food, cordials, stimulants, and tonic medicines. It does the same in respect to depressing causes, such as hunger, cold, and sedatives.

With the aid of this instrument the fact is demonstrated, that the action of the heart may be great when the pulse is small,— that the heart may strike the instrument with force when the pulse scarcely affects the liquid of the hand-sphygmoscope. It affords a remarkable proof that the pulse is one thing and the heart's action another, and teaches that the pulse is only an approximate sign of the state of the heart. It is found also, that while cold at the surface and extremities may depress the pulse, the heart may remain little enfeebled, or even become excited, and that warmth and friction applied to the extremities may cause an excited pulse without there being any accompanying increased force of the heart.

The influence of respiration upon the action of the heart is manifested, in some degree, by the instrument placed over the region of the heart. If the breath be stopped after an ordinary expiration, the movement of the liquid is seen to be increased. If a very long and forcible inspiration be made and the breath then suspended, the movement is somewhat reduced; but when the respiration is again allowed to take its normal course, the movement is seen to be increased for a short time.

Fig. 2.

The sphygmoscope rises during the first sound of the heart and falls at the second.

The sphygmoscope reduced (fig. 2), deprived of its stand, having a level elastic wall instead of protruding one, and having a glass tube with an almost capillary bore, forms a remarkably delicate indicator of the pulse*. It is so delicate in its impressions that it is appreciably affected by the regurgitant wave in the jugular veins, and by the wave in arteries greatly smaller than the radial. From its nicety in manifesting the beat of the blood-wave, it is very valuable, and is called the hand-sphygmoscope.

By means of this hand instrument applied to the arteries, a comparison is readily made between the time of the beat of the heart and the rise of the arteries under the influence of the blood-wave. This instrument is much more delicate than the finger in such an inquiry. The impressions made upon the fingers of two hands fail to be conveyed with sufficient nicety to the mind to tell with certainty the relative time of the beat of the heart and arteries. Except in cases of

Hand-sphygmo-scope.

* Since this instrument was contrived, the author has learned that a sphygmometer of much the same construction was invented some twenty years ago by Mons. le Docteur Hérrison, and that a memoir upon it was presented to the Institute of France. The liquid employed was mercury—too heavy to indicate feeble impulses, and the moveable wall was of gold-beater's skin, which is inelastic. It may be added, that M. Magendie reported against the practical application of the invention.

extreme slowness, the sensations obtained from the two hands impressed at nearly the same time, do not admit of a distinct difference in respect to time being made out. It has been to this very defect that the erroneous idea, that the beat of the heart and the beat of the pulse are synchronous, or nearly so, has owed its origin and continuance.

The hand-sphygmoscope, placed upon the radial artery, shows a rise of the liquid while there is a fall in the sphygmoscope placed over the heart. As the liquid in the one instrument starts from below, the liquid in the other starts from above, and as the liquid in the one reaches the top of its ascent, the liquid in the other reaches the bottom of its descent, to renew their opposing course. The movements in the two instruments at the same instant are always opposed, and the whole time occupied in the movement of one instrument in one direction appears to be occupied by the movement of the other in the opposite direction. The movements *alternate* with as much apparent exactitude as the arms of a well-adjusted balance. When the lapse of time between the beat of the heart and the pulse at the wrist was first observed, suspicion of disease of the aorta was entertained, but the subsequent examination of many persons proved that this alternation was natural. In some twenty persons subjected to examination, the complete alternation has been made out without the shadow of a doubt. These persons were of all ages above childhood, and had the pulse of different degrees of rapidity from 60 to 100.

Hand-sphygmoscopes placed upon the carotid, the brachial, the radial, the femoral, and the dorsal artery of the foot, rise at the same instant, and fall at the same point of time.

These facts prove the existence of two great laws not previously enunciated,—1st, that the heart's beat alternates with the pulse at the wrist ; 2ndly, that the pulse of arteries beyond the chest takes place in all parts at the same instant, and without any appreciable interval.

The pulse, it appears, occurs during the retirement of the heart from the thoracic walls, and the collapse or fall of the arteries takes place during the impulse of the heart. During the rise in the hand-sphygmoscope placed over the arteries, the second sound of the heart has been distinctly heard, and during the fall, the first, softer and more prolonged sound has been easily distinguished.

The horse has been subjected to examination, to learn the relative time of the beat of the heart and arteries, but the respiratory movements and the motions of the animal have hitherto restricted the application of the instruments. However, it has been most distinctly ascertained, by the hand placed upon the heart and upon the plantar artery, that between the beat of these parts there is a decided interval. The slowness of the action of the heart in the horse renders this experiment less open to error than in man. In these experiments upon the horse, Mr. Mavor, the eminent veterinarian, gave his valuable aid.

The sphygmoscope forms a good pneumoscope. It delicately

measures the rise and fall of the chest in respiration. It likewise declares the relative duration of inspiration and expiration, and may thus prove useful in the detection of incipient phthisis, and other pulmonary diseases. When the liquid has attained its highest elevation at the end of inspiration, it immediately begins to fall; but when it has reached the lowest point at the end of expiration, it remains there some instants. The ascent is slower than the descent. After the fall of an ordinary expiration, a forced expiration gives a second fall.

The sphygmoscope may be employed without a stand and is then more portable (fig. 3), but from the want of a fixed basis, and from the motion of the ribs on which it must rest, its manifestations are less extensive and satisfactory. It may be maintained *in situ* with an elastic band placed around the thorax. When employed without a stand, as it must rest upon the ribs, the elastic wall of the chamber should be plain, and not protruding.

Fig. 3.

The hand-sphygmoscope is an exceedingly delicate instrument, but requires great care and nicety in its construction. It may be made by taking about an inch and a half of a gutta percha tube, half an inch in diameter, slightly widening one extremity of it to make a chamber large enough to hold a small horse-bean, and fastening with thread a piece of thin india-rubber, or of Bourgeaud's india-rubber bandage, securely over it for the elastic and moveable wall. The liquid is now supplied, and the glass tube, with a very fine flat bore, say $\frac{1}{10}$th of an inch, and provided with a ring of india-rubber, obtained by cutting off a small portion of a fine india-rubber tube, for a "washer," is now inserted and the instrument is ready for use. The hand-sphygmoscope discovers the blood-wave in regurgitation of the jugular veins; it responds to the radial of the newly-born infant; it rises and falls with the movements of the brain of the infant, though some months old, as that organ rises and falls under the influence of its arteries. There is no doubt that, applied to the fontanelles before delivery, it will inform the obstetrician whether the fœtus be dead or alive, and, in cases of difficult labour, supply important evidence for his guidance.
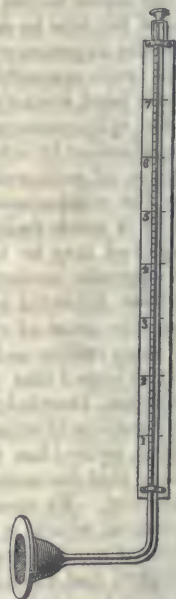


Portable Sphygmoscope.

The hand-sphygmoscope applied to the radial artery, and to the fontanelles of a dying infant three months old, has indicated to the author the influence of respiration upon the circulation. During inspiration, the column of liquid in the tube was found to fall as if sucked down, and during expiration to spring again.

In practical surgery, the hand-sphygmoscope may possibly be employed with advantage, for it will rise with the wave or fluctuation of liquid tumours. It may be placed where the fingers cannot reach.

The rise in the instrument is greater in liquid than in aëriform tumours on account of the compressibility of air, and the fall is more rapid and decided when the contents of the tumour are liquid.

For the most part, the hand-sphygmoscope is best applied simply with the aid of the fingers. It is delicately held between the tips of the thumb, fore- and middle-fingers, the nails resting on the examinee. The elastic wall is on no account to be pressed down with a dead weight upon the vessel. It is to be nicely lowered to the level of the artery when collapsed. When the artery rises, it will strike the elastic wall, and as the chamber is fixed by the fingers, the entire blow is communicated to the liquid and it rises in the tube. During the retirement or collapse of the artery, the elastic wall resumes its level condition and draws the liquid down the tube. This motion of the liquid allows the instrument to be employed though the open end of the tube be dependent. When it is desired to avoid the varying pressure experienced when the instrument is held between the fingers, some such apparatus as was invented by Dr. E. S. Blundell, or an elastic band suitably applied around the wrist, will be useful.

The sphygmoscope is for several purposes rendered more convenient of application by interposing, between the chamber and the glass tube, a piece of india-rubber tube of suitable bore and length. In this way the comparison of the beat of the heart and the pulse of an artery is much facilitated, for the glass tubes of the two instruments employed may be brought parallel and close to each other, so that the opposite motions of the liquids in the two tubes are, by near contrast, rendered easier of observation. In employing this adaptation, care must of course be taken that the india-rubber tube is of the same calibre and length in both instruments.

It is hoped that the sphygmoscope will aid in the acquisition of additional knowledge of the movements and condition of the heart, the situation of which within a case of bone, wisely provided to secure it from injury, has this disadvantage for the physiologist and physician, that the action and condition of the organ are with difficulty made out. By means of the sphygmoscope, that small amount of movement which is manifested at the exterior of the chest may be rendered more appreciable to our senses, and more available for physiological and curative purposes ; and perhaps information may be obtained by this instrument which has hitherto been procurable only by the practice of vivisection.

Park Street, Grosvenor Square, London,
Jan. 12, 1856.

Feb. 28.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

. The Bakerian Lecture.—" On the Electro-dynamic Properties of Metals." By Professor William Thomson, F.R.S.

The Lecturer gave an exposition of the substance of a paper presented by him to the Society under the above title.

The paper consists of five parts, namely :—1. On the Electric Convection of Heat ; 2. On Thermo-electric Inversions ; 3. On the Effects of Mechanical Strain and of Magnetization on the Thermo-electric Qualities of Metals ; 4. On Methods for comparing

and testing Galvanic Resistances, illustrated by Preliminary Experiments on the Effects of Tension and Magnetization on the Electric Conductivity of Metals ; 5. On the Effects of Magnetization on the Electric Conductivity of Iron.

1. In the first part a full account of the experiments, of which the results were communicated to the Royal Society in April 1854 [*], is preceded by a short statement of the reasoning, founded on incontrovertible principles regarding the source of energy drawn upon by a thermoelectric current, which led the author to commence the experimental investigation with the certainty that the property looked for really existed whether he could find it or not. In confirmation of the extraordinary conclusion then announced,—that an electric current in an unequally heated conductor, if its *nominal direction* be from hot to cold through the metal, causes a cooling effect in iron, and a heating effect in copper,—the author describes new experiments which he has recently made, and which are as decisive in leading to the same conclusion as those by which he had first established it. He also describes experiments by which he had recently given an independent demonstration that brass has the same property as copper, and platinum the same quality as iron, with reference to electric convection of heat ; results anticipated [†], one as certain, and the other as highly probable, from the previous results regarding electric convection in copper and iron, and from the known thermo-electric relations between these metals and the others.

2. The phænomenon of thermo-electric inversion between metals, discovered by Cumming, forms the subject of the second part. A mode of experimenting is described, by which inversions may be readily detected when they exist between any two metals, and, when thermometers are available, the temperature of neutrality determined with precision. Various results of its application are mentioned, of which some are shown in the following Table :—

| −14° Cent. | −12°·2 | −1°·5 | 8°·2 | 36° | 38° | 44° | 44° | 47° |
|---|---|---|---|---|---|---|---|---|
| P₃ Brass | P₁ Cadmium | P₁* Silver | P₁ Zinc | P₂ Lead | P₂ Brass | P₂ Tin | Lead Brass | Silver Zinc |

| 53° | 64° | 71° | 72° | 99° | 121° | 130° | 162°·5 | 237° | 280° |
|---|---|---|---|---|---|---|---|---|---|
| P₂ Double wire of Palladium, 11·31 grs., and Copper, 19·41 grs. | P₁ Copper Gold | Silver Gold | Gold Zinc | P₁ Brass | P₁ Lead | P₁ Tin | Iron Cadmium | Iron Silver | Iron Copper |

The number at the head of each column expresses the temperature Centigrade by mercurial thermometers, at which the two metals written below it are thermo-electrically neutral to one another ; and the lower metal in each column is that which passes the other from *bismuth towards antimony as the temperature rises*. $P_1$, $P_2$, $P_3$ denote three particular specimens of platinum wire, used by the author as standards.

[*] See Phil. Mag. July, 1854.
[†] See Phil. Mag. July 1854 ; also " Dynamical Theory of Heat," Part VI. § 135 ; Phil. Mag. vol. xi. p. 292.

It was also found that Aluminium must be neutral to either $P_3$, or Brass, or $P_2$, at some temperature between $-14°$ C. and $38°$ C.; that Brass becomes neutral to Copper at some high temperature, probably between $800°$ and $1400°$; Copper to Silver, a little below the melting-point of silver; Nickel to Palladium, at some high temperature, perhaps about a low red heat; and $P_3$ to impure mercury (that had been used for amalgamating zinc plates), at a temperature between $-10°$ and $0°$. Probably $P_3$ becomes neutral to pure mercury at some temperature below $-10°$ C.

3. In the third part, effects of mechanical strain, and of magnetization on the thermo-electric qualities of metals, are investigated. The author had previously communicated to the Royal Society* results he had obtained regarding the thermo-electric qualities of copper and of iron wires under longitudinal stress, namely, that the former exhibits a deviation towards bismuth, and the latter towards antimony, from the same metal in an unstrained state.

The only kind of stress applicable to a solid which has no directional attributes, is uniform pressure or traction in all directions. Hence it appeared probable to the author that a simple longitudinal stress would induce different thermo-electric qualities in different directions, in any homogeneous non-crystalline metal subjected to it. But he had found (see Phil. Mag. July, 1854) that the thermo-electric effect of longitudinal traction on a wire, either of iron or of copper, is sensible to tests he could readily command, and more so in the case of the former than in that of the latter. He therefore made experiments to test the difference of thermo-electric quality in different directions in a mass of iron under stress, and fully established the conclusion that the thermo-electric quality across lines of traction differs from the thermo-electric quality along lines of traction, as bars of bismuth differ from bars of antimony. The experiments he has already made nearly establish the conclusion that unstrained iron has intermediate thermo-electric quality between those of the two critical directions in iron under distorting stress.

The experiments of Magnus show that wires hardened by wire-drawing have different thermo-electric qualities lengthwise from wires of the same substance softened by annealing. The author has verified, that in copper, iron, and tin, simple traction, leaving permanent elongation, leaves also a thermo-electric effect, the same as Magnus had found by wire-drawing, which is a composite application of longitudinal traction and lateral compression; and that in a variety of metals, namely, iron, copper, brass, tin, platinum, permanent lateral compression (by hammering) leaves still the same thermo-electric effect, as Magnus had found by wire-drawing. In cadmium, not examined by Magnus, and lead, which had not a given result, the experiments now adduced show a thermo-electric effect of hammering, the same as in all the other metals except iron. Zinc wire was also tested, and found to exhibit the same effect as copper, though Magnus had found a reverse quality as due to wire-drawing. The discrepance in this case is probably due to the peculiar effect of

* April 1854. See Phil. Mag. July, 1854.

annealing on zinc wire, making it brittle and crystalline, which might
give a different condition, as the " annealed" in Magnus's experi-
ment, and the "unhammered" in the experiment now adduced.
Setting aside this case, the author concludes that generally the effect
of permanent lateral compression is the same as that of permanent
longitudinal extension, or of hardening by wire-drawing, upon the
thermo-electric quality of a wire placed longitudinally in an electric
circuit; that in iron it is a deviation from the constrained metal
towards bismuth, and that in all the other metals mentioned it is a
deviation towards antimony; and that in copper and iron it is the
reverse of the effect experienced by the same metal while under the
stress that caused the strain. Since no kind of strain, except uni-
form condensation or dilatation in all directions, is free from the
directional attribute, it appeared probable to the author that the
thermo-electric effects remaining in a metal left with a longitudinal
strain, retained after the stress that caused it is removed, must be
different in different directions. He therefore experimented on iron
hardened by longitudinal compression, and found that it deviates
from soft iron towards antimony, or in the contrary way to iron
hardened by longitudinal traction. From this, and from the results
quoted above, it follows that in iron hardened by compression in one
direction, the thermo-electric qualities in this direction differ from
those in lines perpendicular to it, as antimony differs from bismuth;
that the reverse statement applies to iron hardened by traction in one
direction; and that these differing thermo-electric qualities have in
each case the thermo-electric quality of soft iron intermediate between
them.

These various results show that the character of the effect in each
case is decided by *distorting stress* or by *distortion*, and leave entirely
open, and only to be answered by further experiments, the questions:
what is the thermo-electric effect of pressure or traction, applied
uniformly in all directions to a metal? and what is the thermo-
electric effect of a permanent condensation or dilatation remaining in
the metal, when freed from the force by which that condensation or
dilatation was produced?

Experiments are also described, by which the author found that
in soft iron under magnetic force, and in that retaining magnetism,
when removed from the magnetizing force, directions along the lines
of magnetization deviate thermo-electrically towards antimony; and
that directions perpendicularly across the lines of magnetization in
soft iron, deviate towards bismuth, from the unmagnetized metal.
He illustrates this conclusion by an experiment on a riband of iron,
magnetized nearly at an angle of 45° to its length, and heated along
one edge while the other is kept cool. When the two ends, kept
at the same temperature, are put in communication with the elec-
trodes of a galvanometer, a powerful current is indicated, in such
a direction, that if pursued along a rectangular zigzag from edge to
edge through the band, the course is always *from across to along the
lines of magnetization through the hot edge, and from along to across
the lines of magnetization through the cold edge.*

4. In this part of the communication, attempts made by the author to find the effects of various influences on electric conductivities of metals are described. One of these, with a very unsatisfactory method for testing resistances, led to the conclusion that longitudinal magnetization diminishes the conducting quality of iron wire. The general plan for testing resistances, which he subsequently adopted as the best he could find, and which has proved very satisfactory, is next explained ; and as an illustration, a single experiment on the relative effect of an equal longitudinal extension on the resistances of iron and copper wires is described. The conclusion established by this experiment is, that both by extension with the tractive force still in operation, and by permanent extension retained after a cessation of stress, the conductivity of the substance is more diminished in iron than in copper ; or else that it is more increased in copper than in iron, or increased in copper while diminished in iron, if it is not in each metal diminished, as the author is led by a partial investigation of the absolute effect in each metal to believe.

5. The result previously arrived at regarding the effect of longitudinal magnetization on the conductivity of iron is confirmed ; and an experiment that would have been found impracticable by the less satisfactory method, proves the same conclusion for magnetized steel wire, with the magnetizing influence away. Two very different experiments show further, that the electric conductivity of magnetized iron is greater across than along the lines of magnetization. A last experiment, showing that iron gains in conducting power by magnetization across the lines of the electric current, leads to the conclusion that there is a direction inclined obliquely to the lines of magnetization, along which the conductivity of magnetized iron would remain unchanged on a cessation of the magnetizing force.

---

### CAMBRIDGE PHILOSOPHICAL SOCIETY.

Feb. 25.—A paper was read, " On a direct method of estimating Velocities, Accelerations, and all similar magnitudes with respect to Axes moveable in any manner in Space, with applications." By Mr. Hayward, of St. John's College.

The frequent recurrence, in many different investigations of kinematics and dynamics, of exactly corresponding equations, suggests the inquiry whether they do not result from some common principle, from which they may be deduced once for all. An investigation based on this idea forms the first part of this paper, and the result is the method mentioned in the title.

This calculus shows how the variations of any magnitude, capable of representation by a straight line of definite length in a definite direction, and subject to the *parallelogrammic* law of combination, may be *simply* and *directly* determined relatively to any axes whatever. If such a magnitude ($u$) be estimated in a given direction, its intensity in that direction will be represented by the projection on

it of the line which represents $u$. If this given direction be not fixed, but move according to a given law, the projection of $u$ upon it will change by the alteration of its inclination to the direction of $u$; and the rate of that change is easily calculated, whence an expression for the acceleration of the resolved part of $u$ along a given axis as *due to the motion of that axis*. If $u$ itself be variable, its variations may be conceived to be due to an acceleration $f$ in a definite direction, which in the time $dt$ produces a quantity $fdt$ in the direction of $f$ to be combined with $u$ by the parallelogrammic law; hence result expressions for the changes in intensity and direction of $u$. If, $u$ being variable, the variations in its intensity estimated along a given moveable direction be sought, it will consist of two parts : one, that due to the resolved part of $f$ in the given direction; the other, that due to the motion of the axis, which is the same as if $f$ had not existed, or $u$ had been constant : hence expressions for the total acceleration of the resolved part of $u$ along the given moveable axis. If $u$ be resolved along three rectangular axes, these expressions take the forms of familiar kinematical and dynamical equations.

These results furnish immediately expressions for the relative velocities of a point with respect to moving axes when its absolute velocities in their directions are given, and *vice versâ*. They also furnish very ready means of estimating accelerations in variable directions; as, for instance, the radial and transversal accelerations of a point moving in a plane or in space, or the tangential and normal accelerations in the same case. These are some kinematical applications of the calculus.

The dynamical applications form the second part of the paper. Here the general problem of the motion of a system, so far as it is due to *external* forces, is divided into two steps; one from *force* to *momentum*, the other from *momentum* to *velocity*. If the momenta of the particles of a system be reduced like a system of forces, they produce a single *linear momentum* and a single *angular momentum*, just as a system of forces produces a single force and a single couple. The linear momentum is (in our received language) the momentum of the mass of the system collected at its centre of gravity; the angular momentum is a magnitude the constancy of whose intensity in a given axis is equivalent to the assertion of the principle of the conservation of areas for that axis, and the constancy of whose directions determines the " invariable plane " as a plane perpendicular to it. The momentum, whether linear or angular, is a magnitude to which the previous calculus applies, and the resultant force and resultant couple are respectively the accelerators of the two kinds of momentum : hence the equations obtained in the first part, interpreted with respect to these magnitudes, furnish equations in any required form for the determination of the momenta at any instant. The step from force to momentum is independent of the nature of the system, that from momentum to velocity requires the system to be particularized. In the paper the case of an invariable system only is considered, and in particular its motion of rotation about its centre of gravity. The axis of rotation or angular velocity is related in

direction to that of angular momentum, as the radius of the central ellipsoid with which it coincides to the normal at its extremity. Hence an angular momentum constant in intensity and direction, in general gives rise to an angular velocity variable in both respects, and *vice versâ*. The question then becomes, to determine the acceleration of angular velocity due to the motion of the system. This is obtained by determining the acceleration of angular momentum for a line fixed in the body, which is then shown to be a maximum for the normal to the plane containing the axes of angular momentum and velocity ; then the acceleration along this line is the total acceleration of angular momentum due to the motion, and the acceleration of angular velocity determined from it (just as the angular velocity is determined from the momentum) is that due to the motion of the system. Also the acceleration of angular velocity due to the forces is related to the resultant couple and its axis, just as the angular velocity to the angular momentum. Thus the accelerations of angular velocity due both to the motion and to the forces being determined, the intensity and direction of the angular velocity at any time is to be found by combining these effects by integration. The problem is worked out in the case of the axis of the resultant couple being coincident with that of angular momentum, so that this remains fixed. The paper concludes with a simple solution of the problems of Foucault's gyroscope as applied to show the effects of the earth's rotation, the simplicity arising from the method of this paper enabling us at once to refer the motion to those axes (neither fixed in the body nor in space) whose motion it is desired to determine.

April 28.—A paper was read on the Theory of Heat, by Mr. A. A. Harrison of Trinity College.

The object of this paper was to show that there is considerable reason for supposing that radiant heat is identical with light, and that they both consist of vibrations of the ultimate particles of matter.

There is a strong presumption of this from the facts, that every body heated to a certain temperature, dependent only on the nature of the surface, emits light as well as heat ; and that " whenever light manifests itself, heat appears along with it " (Kelland) : the difference between radiant heat and ordinary heat is, that radiant heat is due to vibrations in planes normal to its direction of propagation, and that ordinary heat consists of vibrations in all three dimensions.

The author endeavoured to show, in the first place, that the motions of the particles of matter, which must be caused by friction, or in the union of two gases in combustion, is sufficient of itself to account for the following phænomena of heat :—

I. That a body once heated continues of the same temperature, with the exception of heat lost by radiation, conduction, &c. This follows immediately from the principle, that in any system of particles held together by mutual attractions and repulsions, the *vis viva* is independent of the time, and depends merely on the position of the particles.

II. That bodies expand by heat.

Before proceeding to this, the author argued that in gases the

repulsive force varies inversely as the cube, and not, as usually stated, the simple power of the distance ; that it is not true, without some limitation, that the force varies as the inverse first power, was urged from the fact that such a force would decrease more slowly than one varying as the inverse square, and consequently would be the force observed in astronomical phænomena ; and even the oxygen of the ocean would repel that of the air instead of attracting it. That the force varies as the inverse cube was deduced from the law of elasticity, that the density varies as the pressure ; for if a particle repels other particles with a force varying as the inverse cube, it repels a fixed plane of them with a force varying as the inverse first power. That this is the case may be seen, by considering that though the particle repels particles similarly situated with a force varying as the inverse cube, yet the number of such particles varies directly as the square of the distance, and therefore the whole effect upon the plane varies inversely as the first power. And if this is true for a plane, it is also true for the solid side of the containing vessel ; for any solid may be considered as made up of a succession of planes.

The law being the inverse cube, it follows that in any position the sum of the forces exerted by any particle on two particles, one on each side of it, is least when that particle is half-way between them, and increases the further the particle is removed from the middle point. This is seen directly, for the value of $\dfrac{\mu}{(a+x)^3} + \dfrac{\mu}{(a-x)^3}$ is least when $x=0$, and increases until $x=a$. And therefore, in order to produce the same force, it would be necessary that the mean distance should be increased ; and hence if the particles of any aëriform body be in motion, the force exerted by them would be greater than when at rest ; that is, if the pressure to be supported be constant, the average distance of the particles must increase, and the body must expand.

III. That every aëriform body not in contact with a liquid expands in the same proportion. This was accounted for by the circumstance, that the increase of pressure depends only upon the ratio of the disturbance to the original distance, and not at all upon the absolute distance.

IV. That air and elastic fluids give out heat on compression. By compression the absolute distance of the particles from one another is diminished ; but the absolute motion remaining the same, the relative motion is increased.

V. That the same amount of heat is generated in two gases subjected to the same pressure ; for the absolute distance of the particle in both being diminished in the same proportion, and the absolute motion remaining unaltered, the relative motion is increased in the same proportion in both.

VI. The specific heats are inversely as the atomic weights. Here it was necessary to show that mass is not necessarily proportional to the quantity of matter, as usually stated ; or rather, that a body may have a different mass when considered with regard to the molecular force from what it has with respect to the force of gravity. With

regard to elasticity of gases, the weight of any single particle is so small as not to affect the result. The question remains, whether we know anything of the masses of different particles relatively to this repulsive force. To determine their masses we have these data. In several different gases equivalent volumes under the same pressure occupy the same space, that is (assuming the Daltonian theory, that equivalent volumes contain the same number of particles), that each particle of the two different gases exerts an equal pressure on the adjacent particles : and hence with reference to this law, the mass of a single particle in each of these two different gases is the same, and therefore the " *vis viva* " of equivalent weight or volumes subject to the same motion is the same for both ; that is, the quantity of heat of an equivalent of each is the same, and therefore the specific heat of a given weight is inversely as its equivalent number or atomic weight.

With reference to the phænomena of radiation, it may be shown from theoretical considerations that the inverse cube is the law required. The inverse first is impossible, for then there could be no vibrations. For the same reason the inverse second is impossible (Camb. Phil. Trans. vol. vii. p. 98). The inverse fourth is also impossible, for then there could be no vibrations, and the velocity would be infinite (vol. vi. p. 325). It has also been shown that neither the second nor the fourth would satisfy the conditions of the equations (vol. vii. p. 419). Hence, from the theory of radiation, it is supposed that the luminiferous æther consists of solid particles, attracting one another with a force varying as the inverse square (vol. vii. p. 110), and repelling with a force varying as the inverse cube.

Now from the Daltonian theory, and the law of elastic fluids, it has been shown that the ultimate particles of our atmosphere compose such an æther. But if our atmosphere is the luminiferous æther, we must next inquire whether it does pervade space. Omitting variations of temperature, and merely considering the atmosphere as subject to the two forces of elasticity and gravity, we have for the equation of a column of air on a unit of surface,

$$dp = -\frac{ga^2}{z^2}\rho dz, \text{ or } \frac{1}{p}\frac{dp}{dz} = \frac{kga^2}{z^2}, \text{ where } k = \frac{\rho}{p}.$$

Integrating this, we find that $p$ and $\rho$, though they become extremely small, never vanish ; and therefore, if these laws are absolutely true, our atmosphere does pervade space.

It may be well to obviate the objection, that black substances radiate heat best, and white substances light. This arises from employing the same word radiation to denote two different things : by radiated heat is meant heat given out from a heated body ; by radiated light is meant the secondary radiation from the surface of a body exposed to light.

If Sir J. Leslie's experimental calculation of the heat lost from the sun be correct, there is no need of any theories to account for its generation.

From the foregoing arguments and facts, it was urged that motions and forces, which certainly exist in cases of combustion, would produce phænomena exactly similar to those of heat, and therefore that part of the phænomena usually attributed to heat are due to this motion; and if part of them, probably the whole. And further, that if the phænomena of radiation of heat are explained by this motion of the particles of matter, light is simply radiated heat of considerable intensity; and that imponderable substances, whether under the names of æther, caloric, or phlogiston, are equally imaginary.

Also, a paper was read by Mr. Maxwell on the Elementary Theory of Optical Instruments.

The object of this communication was to show how the magnitude and position of the image of any object seen through an optical instrument could be ascertained without knowing the construction of the instrument, by means of data derived from two experiments on the instrument. Optical questions are generally treated of with respect to the pencils of rays which pass through the instrument. A pencil is a collection of rays which have passed through one point, and may again do so, by some optical contrivance. Now if we suppose all the points of a plane luminous, each will give out a pencil of rays, and that collection of pencils which passes through the instrument may be treated as a *beam* of light. In a pencil only one ray passes through any point of space, unless that point be the focus. In a beam, an infinite number of rays, corresponding each to some point in the luminous plane, passes through any point; and we may, if we choose, treat this collection of rays as a pencil proceeding from that point. Hence the same beam of light may be decomposed into pencils in an infinite variety of ways; and yet since we regard it as the same collection of rays, we may study its properties as a beam independently of the particular way in which we conceive it analysed into pencils.

Now in any instrument the incident and emergent beams are composed of the same light, and therefore every ray in the incident beam has a corresponding ray in the emergent beam. We do not know their path within the instrument, but before incidence and after emergence they are straight lines, and therefore any two points serve to determine the direction of each.

Let us suppose the instrument such that it forms an accurate image of a plane object in a given position. Then every ray which passes through a given point of the object before incidence passes through the corresponding point of the image after emergence, and this determines one point of the emergent ray. If at any other distance from the instrument a plane object has an accurate image, then there will be two other corresponding points given in the incident and emergent rays. Hence if we know the points in which an incident ray meets the planes of the two objects, we may find the incident ray by joining the points of the two images corresponding to them.

It was then shown, that if the image of a plane object be distinct,

flat, and similar to the object for two different distances of the object, the image of any other plane object perpendicular to the axis will be distinct, flat, and similar to the object.

When the object is at an infinite distance, the plane of its image is the *principal focal plane*, and the point where it cuts the axis is the *principal focus*. The line joining any point in the object to the corresponding point of the image cuts the axis at a fixed point called the *focal centre*. The distance of the principal focus from the focal centre is called the *principal focal length*, or simply the *focal length*.

There are two principal foci, &c. formed by incident parallel rays passing in opposite directions through the instrument. If we suppose light always to pass in the same direction through the instrument, then the focus of incident rays when the emergent rays are parallel is the *first* principal focus, and the focus of emergent rays when the incident rays are parallel is the *second* principal focus. Corresponding to these we have first and second focal centres and focal lengths.

Now let $Q_1$ be the focus of incident rays, $P_1$ the foot of the perpendicular from $Q_1$ on the axis, $Q_2$ the focus of emergent rays, $P_2$ the foot of the corresponding perpendicular, $F_1F_2$ the first and second principal foci, $A_1A_2$ the first and second focal centres, then

$$\frac{P_1F_1}{A_1F_1}=\frac{P_1Q_1}{P_2Q_2}=\frac{F_2P_2}{F_2A_2},$$

lines being positive when measured in the direction of the light. Therefore the position and magnitude of the image of any object is found by a simple proportion.

In one important class of instruments there are no principal foci or focal centres. A telescope in which parallel rays emerge parallel is an instance. In such instruments, if $m$ be the angular magnifying power, the linear dimensions of the image are $\frac{1}{m}$ of the object, and the distance of the image of the object from the image of the object-glass is $\frac{1}{m^2}$ of the distance of the object from the object-glass. Rules were then laid down for the composition of instruments, and suggestions for the adaptation of this method to second approximations, and the method itself was considered with reference to the labours of Cotes, Smith, Euler, Lagrange, and Gauss on the same subject.

---

## LI. *Intelligence and Miscellaneous Articles.*

### NOTE ON THE EMPLOYMENT OF INDUCTION APPARATUS. MERCURIAL INTERRUPTOR. BY LÉON FOUCAULT.

IN most kinds of induction apparatus, the inductive current is rendered intermittent by the action of an interruptor which periodically produces contact between the extremities of the circuit.

Amongst all the metals which have hitherto been tried for the arma-
ture of these points of contact, platinum has succeeded best; its high
fusing-point and its slight tendency to oxidation indicating it as pre-
ferable to the ordinary metals for presenting long resistance to the
corrosive action of the spark which passes at each interruption.
Nevertheless when the apparatus is in action for a certain time, pla-
tinum itself is at last attacked ; the surfaces of contact become de-
formed, presenting hollows, or enlargements at each other's expense,
the texture of the metal alters, and at length the interruptor ceases
to perform its functions.    This tiresome result arrives sooner in pro-
portion to the strength of the current, and when this acquires an
intensity above a certain limit, the pieces of the interruptor are sol-
dered together at the first contact, and become quite useless.

In attempting to increase progressively the phænomena of induc-
tion, I met with a difficulty which appeared to be rather serious in
this imperfection of solid contact, and like many others no doubt, I
thought of having recourse to mercury.

In my first essays I perceived that it would be impossible to distri-
bute an intense current continuously with naked mercury ; by this
means the interruption is never sufficiently sudden, the inductive ac-
tion remains weak, the surface of the metal becomes oxidized in a few
moments, and it emits abundant vapours which in course of time do
not fail to exert their deleterious action.  I was thus led to cover the
mercury with a stratum of distilled water, or better still, with a stra-
tum of alcohol, which at once remedies the inconveniences attending
the employment of mercury alone.  Thus the interruption of the
current is produced under alcohol with a dry sound which indicates
a sudden stop ; it gives rise to a strong spark of induction, the alco-
hol soon becomes turbid, but it does not cease condensing the vapours
given off from the point of rupture, at the same time that it protects
the surface of the mercury from oxidation.  The apparatus thus con-
tinues to act with regularity as long as the battery is capable of
maintaining the inducing current.

Regarded in a mechanical point of view, the employment of a liquid
conductor such as mercury introduces a happy modification into the
constitution of the interruptor.  The oscillating piece known as
the *hammer* being no longer limited in its movements by the firm
obstacle which it met with in its anvil, may be replaced by an elastic
plate, which vibrates by its own spring under the influence of an
electro-magnet.  This plate, being curved at its extemity and ter-
minated by a platinum point, closes the circuit of the inductive cur-
rent sixty times in a second, by penetrating more or less into the
mercury.  The contact thus established, notwithstanding its short
duration, is nevertheless perfect ; in itself it only presents a resist-
ance which may be neglected in comparison with those spread
through the circuit, and as the elastic piece vibrates in perfect liberty,
the contacts succeed each other at regular intervals of time, as may
be seen from the uniformity of the sound given by an apparatus in
action.   The series of sparks at the extremity of the induced wire
participates in the same character, and in the crepitating sound which

they give off, the ear recognizes a distinct tone conformable with that of the vibrating plate.

At the same time that it regulates the sparks, this new interruptor, when applied to the ordinary machines in use, is capable of increasing their power to a certain extent. It generally acts so as to render the effects proportionate to the intensities of the current distributed, whence it follows that it is particularly in the employment of strong currents that it presents a marked advantage over the interruptors with solid points of contact.

It is even imprudent with a single machine of ordinary dimensions, to attempt to force the intensity of the inducing current beyond a certain limit, for this will infallibly cause the induced coil to burst internally. But by combining several machines, the sum of the tensions being divided between the various elements of this kind of battery, we may direct upon the whole the action of a number of couples proportional to that of the machines, which increases in the same proportion the distance passed over by the spark between the two extremities of the induced series. This system of combination applies without difficulty to the excellent machines constructed by M. Ruhmkorff, provided they are only combined in pairs.

The hammers are suppressed and replaced by permanent conductors, the two inducing wires are united one after the other, and the interruptor, accompanied by the condenser of the extra-current, is introduced into the circuit. By this precaution each machine must retain its discharge wires at a normal distance; both also retain their commutators, which serve respectively to direct the two parts of the current in such directions that tensions of opposite signs may accumulate at the internal extremities of the induced wires; and lastly, these being put in communication, the external ends which remain free, become the two poles of the system, and furnish sparks at a distance of 30 to 35 millimetres.

When it is desired to place more than two machines under the conduct of the mercurial interruptor, it becomes necessary to isolate the additional machines with particular care. In fact, in comparison with the very strong tensions which are manifested in the induced wire in the neighbourhood of the extremities, the inducing wire which penetrates the axis of the coils must be regarded as an inert conductor, and if this conductor approaches the induced coils at points more or less distant from those in which the tensions are null, it presents a complete course for the discharge. It is therefore important to effect an absolute isolation between the inducing wire and the interior of the induced coil in the additional machines, and this isolation is effected by slipping a glass tube into the annular space which separates the two concentric coils. As soon as this condition was fulfilled by the care of M. Ruhmkorff, four combined machines gave the effects of tension, which might have been expected, and the sparks darted across an interval of 7 or 8 centimetres.—*Comptes Rendus,* July 7, 1856, p. 44.

NOTE ON COLONEL SABINE'S PERIODIC AND NON-PERIODIC TEM-
PERATURE VARIATIONS AT TORONTO (PHIL. TRANS. 1853).
BY S. M. DRACH, ESQ., F.R.A.S.

*To the Editors of the Philosophical Magazine and Journal.*
GENTLEMEN,

In page 152, the *Annual* formula is convertible to

$t_x = 44°·23 - 21°·81 \sin (a + 81° \ 27' = a') - 1°·06 \cos (2a + 161° \ 32'$
$\quad = 2a' - 1° \ 22') - ·80 \cos (3a + 257° \ 42' = 3a' + 13° \ 21')$
$\quad + ·22 \cos (4a + 307° \ 27' = 4a' - 18° \ 21') - ·88 \sin (5a + 50°·41$
$\quad = 5a' + 3°·26) + ·325 \cos 6a = 44° \ 23' - 21°·81 \sin a' - 1·06 \cos 2a'$
$\quad - ·80 \cos 3a' + ·22 \cos 4a' - ·88 \sin 5a'$ nearly,

at 30° a month, 81° 27' = 2 months and 21 days; which, counted
backwards from January 15, brings us to the third week in October,
the epoch of mean annual temperature.

Would therefore the meteorological year from October year *n* to
September year *n* + 1 inclusive produce more approximative results?
Oct. 13, 1856. S. M. DRACH.

ON A ZEOLITIC MINERAL (ALLIED TO STILBITE) FROM THE ISLE
OF SKYE, SCOTLAND. BY J. W. MALLET, PH.D.

The specimen to which the following description refers has been
in my possession for several years, and has attached to it a lable
bearing the name "Hypostilbite;" but analysis shows it to be a
mineral quite distinct from Beudant's hypostilbite of the Faroe
Islands, and differing also from both stilbite proper and epistilbite.

It occurs as a mass of minute crystals, resembling white loaf-
sugar, breaking easily, and crushing under the fingers into a coarsish
crystalline powder. The separate grains, viewed under the micro-
scope, appear as single prismatic crystals, or little groups of three or
four, nearly transparent, colourless, and with a pearlylustre, especially
on two opposite faces,—closely resembling stilbite in fact in general
appearance.

The crystalline form could not be satisfactorily made out, but
seemed to be monoclinic. Hardness a little greater than that of
calcite. Specific gravity = 2·252.

Strong muriatic acid poured over the pulverized mineral at night
had the next morning formed a distinct jelly.

On analysis the following results were obtained :—

|  |  | Atoms. |
| --- | --- | --- |
| Silica | 53·95 | 1·191—3 |
| Alumina | 20·13 | ·392—1 |
| Lime | 12·86 | ·459—1·17 |
| Magnesia | trace |  |
| Potash (with a little soda) | ·87 |  |
| Water | 12·42 | 1·380—3·52 |
|  | 100·23 |  |

Neglecting the small quantity of alkali, these numbers lead us nearly to the formula

$$2(CaO, SiO^3) + 2(Al^2 O^3, 2SiO^3) + 7HO,$$
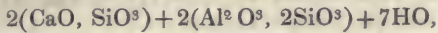
which differs completely from that of stilbite,

$$CaO, SiO^3 + Al^2O^3, 3SiO^3 + 6HO,$$
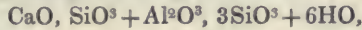
or that of epistilbite,

$$CaO, SiO^3 + Al^2 O^3, 3SiO^3 + 5HO.$$
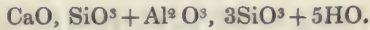
The per-centage of water is also far too small for hypostilbite.

The mineral appears to be a distinct one, and does not seem to have resulted from the gradual decomposition or change of any other; but it is perhaps scarcely desirable to add to the already numerous names of stilbite-like minerals by adopting a new one for this substance until additional analyses of these nearly related species shall permit of their more accurate classification.—*Silliman's American Journal for* September 1856, p. 179.

## METEOROLOGICAL OBSERVATIONS FOR SEPT. 1856.

*Chiswick.*—September 1. Very fine: thunder and lightning at night, but without rain. 2. Shower: very fine: clear. 3, 4. Heavy dew in the mornings: very fine. 5. Slight fog: cloudy and fine. 6. Slight fog: rain at night. 7. Very fine. 8. Foggy: very fine. 9. Dense fog: very fine: cloudy. 10. Slight haze: very fine. 11. Overcast. 12. Cloudy: very fine: rain. 13. Cloudy. 14. Clear and fine. 15. Fine: overcast. 16. Clear: fine. 17. Overcast: rain. 18. Rain: very fine. 19. Clear, quite cloudless: very fine. 20. Very clear: cloudy and cold: slight frost at night. 21. Fine: rain. 22. Clear: showery: fine. 23. Clear: cloudy: bright sun at intervals. 24. Clear: heavy showers. 25. Clear: dense clouds: fine. 26. Fine: rain at night. 27. Heavy rain. 28. Rain: heavy showers. 29. Cloudy and fine. 30. Very fine: cloudy: fine at night.

Mean temperature of the month ............................... 54°·41
Mean temperature of Sept. 1855 ............................... 56 ·11
Mean temperature of Sept. for the last thirty years ......... 56 ·95
Average amount of rain in Sept. ................................. 2·455 inches.

*Boston.*—Sept. 1—4. Fine. 5. Cloudy. 6. Fine. 7. Fine: rain A.M. 8, 9. Fine. 10. Cloudy. 11. Cloudy: rain A.M. 12. Cloudy. 13. Cloudy: rain A.M. and P.M. 14. Fine. 15. Cloudy. 16. Fine. 17. Cloudy: rain P.M. 18, 19. Fine. 20. Cloudy. 21, 22. Fine: rain P.M. 23. Cloudy. 24—26. Fine. 27—29. Cloudy: rain A.M. and P.M. 30. Cloudy.

*Sandwick Manse, Orkney.*—Sept. 1. Clear A.M.: clear, aurora P.M. 2. Bright A.M.: clear P.M. 3. Clear A.M.: cloudy P.M. 4, 5. Clear A.M. and P.M. 6. Bright A.M.: cloudy P.M. 7. Drizzle A.M.: damp P.M. 8. Damp A.M.: fog P.M. 9. Fog A.M. and P.M. 10. Fog A.M.: showers P.M. 11, 12. Bright A.M.: showers P.M. 13. Showers, bright A.M.: cloudy P.M. 14. Drizzle, bright A.M.: showers P.M. 15. Showers A.M. and P.M. 16. Showers A.M.: rain, lightning P.M. 17. Showers A.M.: clear P.M. 18. Sleet showers A.M.: showers P.M. 19. Cloudy A.M.: showers P.M. 20. Showers A.M.: cloudy P.M. 21. Bright A.M.: showers P.M. 22. Rain A.M.: showers P.M. 23. Bright A.M.: showers P.M. 24. Rain A.M.: showers P.M. 25. Showers A.M. and P.M. 26. Clear A.M.: clear, aurora P.M. 27. Hoar-frost A.M.: showers, aurora P.M. 28. Damp A.M.: cloudy P.M. 29. Showers A.M. and P.M. 30. Showers A.M.: clear P.M.

Mean temperature of Sept. for previous twenty-nine years ... 52°·28
Mean temperature of this month ................................. 50 ·83
Mean temperature of Sept. 1855 ................................. 52 ·74
Average quantity of rain in Sept. for previous sixteen years ... 2·82 inches.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.*

| Days of Month. 1856. Sept. | Barometer. Chiswick Max. | Chiswick Min. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 2¼ p.m. | Thermometer. Chiswick Max. | Chiswick M'n. | Boston 8¾ a.m. | Orkney, Sandwick 9¼ a.m. | Orkney, Sandwick 8¾ p.m. | Wind. Chiswick 1 p.m. | Boston | Orkney, Sandwick | Rain. Chiswick | Boston | Orkney, Sandwick |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. | 29·842 | 29·787 | 29·40 | 29·77 | 29·86 | 72 | 44 | 53 | 54½ | 48½ | sw. | calm | calm | | | |
| 2. | 30·174 | 29·910 | 29·50 | 29·96 | 29·77 | 67 | 33 | 57 | 50 | 56½ | n. | nw. | nw. | ·01 | | ·06 |
| 3. | 30·255 | 30·207 | 29·81 | 29·99 | 29·66 | 73 | 37 | 53 | 50 | 56 | nw. | nw. | sse. | | | |
| 4. | 30·196 | 30·095 | 29·73 | 29·87 | 29·85 | 75 | 34 | 53 | 61½ | 54 | se. | calm | s. | | | |
| 5. | 29·984 | 29·863 | 29·58 | 29·95 | 30·03 | 71 | 37 | 61 | 56 | 52 | e. | se. | se. | ·12 | ·16 | ·05 |
| 6. | 29·742 | 29·597 | 29·34 | 30·02 | 29·96 | 70 | 42 | 65 | 56½ | 53½ | sw. | e. | ese. | | | |
| 7. ☽ | 29·866 | 29·738 | 29·28 | 29·84 | 29·85 | 72 | 38 | 57·5 | 53½ | 53½ | w. | w. | e. | | | ·02 |
| 8. | 29·893 | 29·891 | 29·46 | 29·90 | 29·97 | 74 | 40 | 54·5 | 56 | 51 | se. | wnw. | e. | | | ·04 |
| 9. | 29·877 | 29·788 | 29·45 | 29·92 | 29·89 | 72 | 52 | 60 | 53½ | 52½ | e. | e. | se. | | | ·07 |
| 10. | 29·994 | 29·869 | 29·47 | 29·91 | 30·05 | 74 | 48 | 63·5 | 57 | 50½ | ne. | ene. | w. | | ·02 | ·08 |
| 11. | 30·140 | 30·089 | 29·66 | 30·12 | 30·22 | 72 | 47 | 59 | 55½ | 50 | ne. | n. | calm | | ·12 | ·08 |
| 12. | 30·142 | 30·066 | 29·72 | 30·12 | 29·95 | 72 | 46 | 58 | 55 | 52½ | nw. | n. | wsw. | | ·02 | ·21 |
| 13. | 30·079 | 29·970 | 29·57 | 29·95 | 30·04 | 66 | 45 | 58 | 54 | 51½ | se. | nw. | w. | | | ·80 |
| 14. ○ | 30·224 | 30·123 | 29·70 | 29·89 | 29·79 | 67 | 35 | 59·5 | 55½ | 53 | sw. | n. | wsw. | ·11 | | ·13 |
| 15. | 30·186 | 30·126 | 29·64 | 29·69 | 29·61 | 69 | 44 | 59·5 | 50 | 50 | w. | sw. | w. | | | ·31 |
| 16. | 30·130 | 30·092 | 29·60 | 29·76 | 29·60 | 68 | 41 | 57·5 | 52 | 49 | sw. | nw. | ssw. | | | ·12 |
| 17. | 29·952 | 29·798 | 29·40 | 29·38 | 29·43 | 67 | 48 | 57·5 | 50½ | 46 | nw. | wsw. | wnw. | ·10 | ·07 | ·06 |
| 18. | 29·860 | 29·836 | 29·37 | 29·53 | 29·63 | 64 | 37 | 52·5 | 46 | 46½ | nw. | w. | wnw. | ·01 | | ·03 |
| 19. | 29·965 | 29·851 | 29·43 | 29·83 | 29·98 | 60 | 36 | 45 | 50½ | 46 | n. | nw. | wnw. | | | ·37 |
| 20. | 30·043 | 29·997 | 29·60 | 30·11 | 30·04 | 58 | 29 | 46·5 | 49 | 48 | w. | nnw. | w. | | | ·23 |
| 21. ☾ | 30·042 | 29·616 | 29·50 | 29·69 | 29·41 | 66 | 41 | 51 | 49 | 49 | sw. | sw. | e. | ·12 | ·08 | ·25 |
| 22. | 29·510 | 29·244 | 29·05 | 29·17 | 29·23 | 65 | 47 | 51 | 51½ | 50 | sw. | ssw. | e. | ·18 | | ·09 |
| 23. | 29·235 | 29·175 | 28·68 | 29·22 | 29·25 | 66 | 45 | 56 | 52½ | 50 | sw. | sw. | ne. | ·04 | | |
| 24. | 29·231 | 29·070 | 28·75 | 29·28 | 29·30 | 66 | 42 | 52 | 50 | 48 | sw. | wsw. | nne. | ·20 | | |
| 25. | 29·510 | 29·241 | 28·74 | 29·28 | 29·33 | 63 | 39 | 52 | 51 | 41 | sw. | sw. | nne. | ·05 | | |
| 26. | 29·657 | 29·374 | 29·15 | 29·36 | 29·48 | 63 | 45 | 47·5 | 48 | 45 | s.+ | sw. | calm | ·27 | | |
| 27. | 29·138 | 28·968 | 28·83 | 29·61 | 29·71 | 55 | 46 | 55 | 49 | 47 | s. | se. | ne. | ·61 | ·13 | ·07 |
| 28. | 29·002 | 28·722 | 28·50 | 29·62 | 29·56 | 60 | 46 | 56 | 45 | 48½ | sw. | n. | nne. | ·13 | ·52 | ·04 |
| 29. ● | 29·449 | 29·137 | 28·65 | 29·56 | 29·60 | 63 | 45 | 53 | 48 | 45½ | sw. | w. | nne. | ·02 | ·18 | ·04 |
| 30. | 29·789 | 29·566 | 29·05 | 29·61 | 29·67 | 65 | 41 | 54 | 49 | 45½ | sw. | | calm | ·02 | ·12 | ·03 |
| Mean. | 29·837 | 29·690 | 29·32 | 29·730 | 29·724 | 67·16 | 41·67 | 55·0 | 51·98 | 49·68 | | | | 1·99 | 1·42 | 3·18 |

LII. *On Slaty Cleavage, and the Distortion of Fossils. By the Rev.* SAMUEL HAUGHTON, *Professor of Geology in the University of Dublin\*.*

THE first person, so far as I know, who distinctly put forward a purely mechanical theory of slaty cleavage, was the late Mr. Sharpe, in two papers read before the Geological Society of London, December 2, 1846, and November 1, 1848. These papers are published in the third and fifth volumes of the Quarterly Journal of that Society, and form a most valuable basis of well-observed facts on which to establish a mechanical theory of slaty cleavage. At the close of the second of these papers, Mr. Sharpe sums up his observations as follows :—

"Thus all our observations and deductions ultimately converge to the conclusion, that the cleavage must be attributed to pressure caused by the elevation of great masses of rock under conditions of which we are ignorant. And if to this conclusion it should be objected that no similar results can be produced by experiment, I reply that we have never tried the experiment with a power at all to be compared with that employed; and that this may be one of many cases where our attempts to imitate the operations of nature fail, owing to the feebleness of our means, and the shortness of the period during which we can employ them."

The objection thus alluded to by Mr. Sharpe has been completely removed by the illustrative experiments of Professor Tyndall, who deserves the thanks of geologists for the light he has thrown upon this obscure question. In common, I believe,

with many geologists I adopted Mr. Sharpe's views, and have from time to time collected data for forming a more precise mechanical theory than that put forward in general terms by Mr. Sharpe. I was, in short, convinced that if the theory were true, it would bear accurate numerical investigation, and admit of being stated with great precision. With a view to this numerical testing of the theory, I have accumulated a great number of observations and measurements of distorted fossils, which I think are sufficient to enable me to state precisely the laws of slaty cleavage and distortion of fossils.

I shall now proceed to state the results I have arrived at, and to illustrate them by detailed examples; and I shall afterwards point out the extent to which Mr. Sharpe's results agree with my own, and how far they anticipate them.

The first fact observed by me was the following :—

I. *If the trace or intersection of the plane of cleavage and plane of bedding be drawn, the greatest distortion or elongation of the fossils lying in the plane of bedding is parallel to this intersection.*

In explanation of this law, it should be stated that each species of fossil has a certain normal undistorted form which may be ascertained, and that the measurements of the different parts bear, within certain narrow limits, a constant ratio to each other; so that when we examine a distorted fossil, we can calculate the amount of relative extension or compression it has undergone in any direction. Suppose, for example, that we draw any two lines at right angles to each other, and that the ratio of M to N is the ratio of the measurements of a known fossil shell taken parallel to those two directions, and that the ratio of $m$ to $n$ is the ratio of the measurements of the distorted fossil taken in the same directions; then, if we denote by $\rho$ the distortion parallel to M, we shall have

$$\rho = \frac{m}{n} \times \frac{N}{M}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If the fossil had been originally of a circular shape, it would become an ellipse by distortion, and $\rho$ would be the ratio of two rectangular diameters, which in one case become the axes of the ellipse; and it is plain, from the properties of an ellipse, that the maximum value of the fraction denoted by $\rho$ will correspond with the axes of the ellipse.

I have calculated the value of $\rho$ repeatedly, from measurements made on fossils from different districts of the south of Ireland, North Wales, Cornwall, and Devonshire, and have always found that the major axis of the ellipse of distortion is parallel to the intersection of the planes of cleavage and bedding. Having thus obtained a tangible starting-point for my theory, I began to

examine what effect the angle between the planes of cleavage and bedding produced on the amount of distortion. The following law expresses the result of my measurements:—

II. *The distortion of fossils produced by cleavage—estimated in a given direction, such as parallel to the intersection of the planes of cleavage and bedding—varies with the angle between these planes, being greatest when the angle is greatest, and least when the angle is least.*

The distortion depending upon the angle between the planes of cleavage and bedding may be calculated numerically from the following expression:—

$$\Delta = \sqrt{P \sin^2 \phi + Q \cos^2 \phi}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which $\Delta$ denotes the distortion parallel to the intersection of cleavage and bedding, $\phi$ the angle between these planes, and P, Q numbers; P being always greater than Q. The mode of determining the numbers P and Q will be presently explained.

In addition to the two laws of distortion already mentioned, there is a third law, which is so easily observed that it has forced itself on the notice of every person who has paid even a slight attention to this interesting subject. It may be stated as follows:—

III. *The compression in a cleaved rock is greatest in a direction perpendicular to the planes of cleavage.*

Having ascertained satisfactorily the truth of the laws of distortion just given, I proceeded to form a general theory by the aid of the following considerations.

It has been beautifully proved by Cauchy, that if a perfectly homogeneous, uncrystalline mass be subjected to pressures, twists, strains, &c. of any kind, it will settle into a condition of equilibrium, subject to the following law:—Imagine a small sphere in the mass before distortion; this sphere will become an ellipsoid after distortion; and if the distortions in the principal planes be known, we can calculate the forces which produced the distortion in the mass.

This hypothetical problem in mechanics, solved by Cauchy, is illustrated by the phænomena of cleaved rocks; and since, by the measurement of the distortion of the fossils, we can ascertain the amount of distortion in each plane of bedding, we can readily obtain data for calculating the axes of the ellipsoid of distortion, and so obtain an idea of the direction and magnitude of the force which has cleaved the rocks and distorted the fossils. The mode of determining the axes of the ellipsoid is as follows:—Since, by the first law, the intersection of the planes of cleavage and bedding is an axis of the ellipse of distortion in all planes of bedding, it must be an axis of the ellipsoid. Also it is certain, from the third law, that the compression is a maximum perpendicular to

the planes of cleavage; from this fact we may infer that a line perpendicular to the plane of cleavage is also an axis of the ellipsoid, and that the third axis will lie in the dip of the cleavage plane and perpendicular to the other two axes.

If we assume the axes of the ellipsoid to be $a$, $b$, $c$, the axis of $x$ being the intersection of cleavage and bedding, the axis of $z$ perpendicular to the cleavage plane, and the axis of $y$ in the dip of the cleavage plane; if $\phi$, $\phi'$ denote the angles made with the plane of cleavage by two planes of bedding, both passing through the axis of $x$; and if $\dfrac{1}{\rho}$, $\dfrac{1}{\rho'}$ be the distortions in these planes estimated parallel to the intersection of cleavage and bedding, then it is easy to see that we have the following equations from the nature of an ellipsoid :—

$$\left. \begin{aligned} \frac{\sin^2 \phi}{c^2} + \frac{\cos^2 \phi}{b^2} &= \frac{1}{\rho^2} \\ \frac{\sin^2 \phi'}{c^2} + \frac{\cos^2 \phi'}{b^2} &= \frac{1}{\rho'^2} \end{aligned} \right\} \quad \cdots \quad (3)$$

Solving these equations for $\dfrac{1}{b}$ and $\dfrac{1}{c}$, we find—

$$\frac{1}{b} = \sqrt{ \frac{\left( \dfrac{\sin \phi}{\rho'} + \dfrac{\sin \phi'}{\rho} \right) \left( \dfrac{\sin \phi}{\rho'} - \dfrac{\sin \phi'}{\rho} \right)}{\sin (\phi + \phi') \sin (\phi - \phi')}}, \quad \cdots \quad (4)$$

$$\frac{1}{c} = \sqrt{ \frac{\left( \dfrac{\cos \phi'}{\rho} + \dfrac{\cos \phi}{\rho'} \right) \left( \dfrac{\cos \phi'}{\rho} - \dfrac{\cos \phi}{\rho'} \right)}{\sin (\phi + \phi') \sin (\phi - \phi')}}. \quad \cdots \quad (5)$$

From equations (4) and (5) we can calculate $\dfrac{1}{b}$ and $\dfrac{1}{c}$, which are the distortions in the dip of cleavage and perpendicular to cleavage, referred to the distortion in the intersection of cleavage and bedding as unity. All that is requisite, in order to know the complete form of our ellipsoid of distortion, is to ascertain the distortion of fossils in two planes making different angles with the plane of cleavage. This I have succeeded in doing in a few instances, which no doubt could easily be multiplied if the attention of observers were directed to the subject. I shall now mention a few of the instances in order.

EXAMPLE 1.—*Carboniferous Slate of Ardoginna near Ardmore, Co. Waterford.*

The fossils made use of to determine the distortion in these slates were *Orthis crenistria*, *Athyris concentrica*, and *Spirifer*

*clathratus.* The normal measurements assumed for these fossils were—

|  | Length. | Breadth. |
|---|---|---|
| *Orthis crenistria* . . . | 1 | 1 |
| *Athyris concentrica* . . | 294 | 341 |
| *Spirifer clathratus* . . | 1 | 2 |

When the angle between cleavage and bedding was 44°, I found from two distinct specimens of *Orthis crenistria,* one lying with its hinge-line parallel to the intersection of cleavage and bedding, and the other with its hinge perpendicular to this line,—

$$\phi = 44°, \quad \frac{1}{\rho} = 1·860 \quad \textit{Orthis crenistria}$$

$$\frac{1}{\rho} = 1·820 \quad \textit{Orthis crenistria}$$

$$\text{Mean} \quad . \quad . \quad \overline{1·840}$$

When the angle between the planes of cleavage and bedding became 10°, I found as follows, expressing by the numbers hundredths of half-inches :—

| *Athyris concentrica* . | Length distorted | =213 |
|---|---|---|
|  | Breadth ... | =269 |
| *Spirifer clathratus* . | Length distorted | =136 |
|  | Breadth ... | =300 |

Calculating the distortion from these data and the normal measurements before given, by means of equation (1), we find—

$$\phi' = 10°, \quad \frac{1}{\rho'} = 1·088 \quad \textit{Athyris concentrica}$$

$$\frac{1}{\rho'} = 1·102 \quad \textit{Spirifer clathratus}$$

$$\text{Mean} \quad . \quad . \quad \overline{1·095}$$

Introducing the values of $\phi$, $\phi'$, $\rho$, $\rho'$ here given into equations (4) and (5), we obtain—

$$\left. \begin{array}{l} \frac{1}{a} = 1 \\ \frac{1}{b} = 1·026 \\ \frac{1}{c} = 2·426 \end{array} \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}$$

From these results I infer that the slate of Ardoginna has been relatively compressed perpendicular to its cleavage nearly two and a half times, and that this result has been brought

about by a force acting very nearly in the line perpendicular to cleavage, because there is scarcely any difference in the amount of distortion in different directions in the plane of cleavage itself, as is shown by the close agreement of the axes $a$ and $b$. The ellipsoid of distortion in this case is nearly an oblate ellipsoid of revolution, whose equatorial diameter is nearly two and a half times its polar diameter.

The slate of Ardoginna is merely a compressed and hardened mud; it is a bluish shale, with occasional black partings.

### EXAMPLE 2.—*Carboniferous Slate of South Petherwin, Cornwall.*

The next example of cleavage which I shall select is the carboniferous slate of South Petherwin, near Launceston in North Cornwall. The slate of this district and its fossil remains have a striking resemblance to the carboniferous slate beds of many parts of the south of Ireland, which I believe to lie on the same geological horizon. The fossils I have used to determine the amount of cleavage are *Spirifer disjunctus* and *Athyris concentrica*. The former of these fossils is closely allied to *Spirifer clathratus*, of which it is probably only a variety. In calculating the distortion, I have assumed for the normal proportions of this fossil—

Length . . . . . . . . . . . =189
Breadth of hinge-line . . . . . =474
Length of rib making $60°$ with hinge =208

For *Athyris concentrica* I have used the normal proportions already given. The data obtained as the mean of many measurements are as follow:—

$$\phi = 17°, \quad \frac{1}{\rho} = 1\cdot480$$

$$\phi' = 12°, \quad \frac{1}{\rho'} = 1\cdot262.$$

From these data we obtain, by means of equations (4) and (5),—

$$\left.\begin{array}{l} \dfrac{1}{a} = 1 \\[1mm] \dfrac{1}{b} = 0\cdot990 \\[1mm] \dfrac{1}{c} = 3\cdot889 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

This result is very similar to that already found for the carboniferous slate of the Co. Waterford, and proves that the ellipsoid of distortion is *nearly* an oblate ellipsoid, whose polar

axis is little more than one-fourth of its equatorial axis. The nearly equal amount of compression or extension along the axes *a* and *b*, or in the strike and dip of the cleavage planes, is a fact that continually reappears in different districts, and produces a remarkable result in the appearance of the fossils ; viz. that in the plane of cleavage the fossils appear to have undergone little relative distortion, although in some cases, as at Tintagel, they have undergone a considerable extension, preserving their relative normal proportions.

### EXAMPLE 3.—*Carboniferous Slates of Tintagel, Cornwall.*

These slates are somewhat lower down in the geological sequence than those of Petherwin, from which they differ remarkably in appearance, being glossy, and of a greenish-yellow colour. Much of this difference may, however, arise from the fact of their being much more cleaved, and retaining traces of having been exposed to much greater pressure.

The fossils used for the calculation of the amount of distortion were the specimens of *Spirifer disjunctus,* described by Mr. Sharpe, and now deposited in the Geological Museum in Jermyn Street. I have used in my calculation the normal measurements already given.

The following are the results obtained from many measurements :—

$$\phi = 0, \qquad \frac{1}{\rho} = 1\cdot494 \quad \textit{Spirifer disjunctus,}$$

$$\phi' = 12°, \qquad \frac{1}{\rho'} = 2\cdot514 \quad \textit{Spirifer disjunctus.}$$

From these data I readily obtain, by equations (4) and (5),—

$$\left.\begin{aligned} \frac{1}{a} &= 1 \\ \frac{1}{b} &= 1\cdot494 \\ \frac{1}{c} &= 9\cdot839 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (C)$$

We here obtain evidence of an enormous amount of compression perpendicular to the planes of cleavage, being, in fact, relatively *ten* times greater than the compression in the cleavage plane. I should be induced to infer from this result, that the mud or substance from which the rock was formed must have been in a very peculiar condition at the time of compression, perhaps like one of those bulky hydrated precipitates of alumina or peroxide of iron so familiar to the chemist, or like the mud cloud depositing

itself in a very deep sea, so ingeniously imagined lately by Mr.
Babbage. Whatever may have been the primary conditions
under which this compression took place, it is certainly interest-
ing to have an exact numerical measure of its amount.

### Example 4.—*Lingula Beds of the Lower Silurian epoch, Abereiddy Bay, Pembrokeshire.*

These beds consist of a soft, black, carbonaceous shale, and
present the appearance of having been considerably compressed.
There is no distortion of fossils in the cleavage plane itself, a fact
that falls in with what I have before noted of the shales of Water-
ford and South Petherwin. The fossils on which I have made
my calculations are *Lingula Phillipsii* (Salter)* and *Calymene
duplicata.* The normal shapes of these fossils are here given :—

*Lingula Phillipsii* . Length $=184$
Breadth $=173$

*Calymene duplicata* . Length of body . . . $=121$
Length of tail . . . . $=64$
Maximum breadth of body $=143$
Breadth of body two-thirds ⎫
down from the thorax. ⎭ $=131$
Breadth of tail . . . $=108$

The following data were obtained by the measurement of the
distorted specimens of these fossils :—

$$\phi = 0, \quad \frac{1}{\rho} = 1 \qquad \textit{Lingula Phillipsii}$$

$$\phi' = 13°, \quad \frac{1}{\rho'} = 1·950 \quad \textit{Lingula Phillipsii}$$

$$\begin{array}{ll} 1·893 & \textit{Lingula Phillipsii} \\ 1·846 & \textit{Lingula Phillipsii} \\ 1·725 & \textit{Calymene duplicata} \\ 1·732 & \textit{Calymene duplicata} \end{array}$$

Mean . . $\overline{1·829}$

From these data we obtain, as before,—

$$\left.\begin{array}{l} \dfrac{1}{a} = 1 \\[2mm] \dfrac{1}{b} = 1 \\[2mm] \dfrac{1}{c} = 6·881 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

* I would here express, once for all, the great obligations I have been
under to Mr. J. W. Salter of the Geological Survey, Jermyn Street. His

This amount of compression is greater than I found for any other slates, excepting the remarkable slates of Tintagel and Garth.

EXAMPLE 5.—*Green Grits of Llyn Padarn, Llanberis, Lower Silurians lying below the Trap Beds of Snowdon.*

In this case I was enabled to obtain measurements of distorted fossils in three distinct planes, and thus secure a kind of *experimentum crucis* for the mechanical theory of cleavage. The fossil I made use of is *Orthis expansa*, of which the following are the normal measurements :—

*Orthis expansa* . Breadth of hinge . . . . = 238
Breadth of shell . . . . = 289
Length of shell . . . . . = 274
Breadth of vascular impression = 143
Length of vascular impression = 144

By these measurements and those of the distorted fossils, I obtained the following :—

$$\phi = 33°, \quad \frac{1}{\rho} = 1·433$$

$$\phi' = 43 , \quad \frac{1}{\rho'} = 1·552$$

$$\phi'' = 90°, \quad \frac{1}{\rho''} = 1·961.$$

The third of these measurements gives a direct value of $\frac{1}{c}$, viz. 1·961, with which the value calculated from the first two data should agree. The agreement is very good, as appears from the following :—

$$\left.\begin{array}{l} \frac{1}{a} = 1 \\[2mm] \frac{1}{b} = 1·195 \\[2mm] \frac{1}{c} = 1·881 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(E)}$$

It is remarkable that the compression in this case, perpendicular to the cleavage planes, is very small compared with the compressions determined in the slates already discussed. This doubtless arises from the fact, that sandstones resist the cleavage action or pressure better than slate or mud. This singular fact, of a less compression existing in sandstones than in slates, and

accurate knowledge of the forms of the palæozoic fossils was of the greatest service to me, and he allowed me free access to all the specimens in the Museum, and assisted my investigation greatly by his judgement.

a proportionably less-developed cleavage, is a solid argument in support of the mechanical theory of cleavage.

In addition to the examples already given, I have measured several others in which I could only obtain a single position of the plane of bedding in relation to the cleavage plane; this is not sufficient to determine the three axes of the ellipsoid of distortion; but if we assume, as is probable, that this ellipsoid is one of revolution, we can readily obtain an approximate value of the compression due to cleavage as follows :—Assuming $b$ equal to unity in one of the equations (3), we find, after some simple reductions,—

$$\frac{1}{c} = \frac{\sqrt{\left(\frac{1}{\rho} + \cos\phi\right)\left(\frac{1}{\rho} - \cos\phi\right)}}{\sin\phi}. \quad . \quad . \quad . \quad (6)$$

The following examples of the application of equation (6) are taken from my note-book :—

EXAMPLE 6.—*Silurian Black Slates of Moel Benddu, Dinas Mowddy, South of Bala, North Wales.*

The fossil whose distortion was measured in this case was the *Asaphus Powisii*, and its normal shape was assumed as follows :—

> *Asaphus Powisii* . Length of head = 338
> Length of body = 386
> Length of tail = 448
> Breadth of body = 838

From these measurements, compared with the distorted specimens from Moel Benddu, I found—

$$\phi = 65°, \quad \frac{1}{\rho} = 3\cdot020 \quad \overset{\textit{Asaphus Powisii.}}{\text{whole fossil}}$$

$$
\begin{aligned}
&3\cdot230 \quad \text{tail}\\
&2\cdot562 \quad \text{body}\\
&3\cdot265 \quad \text{head}\\
&2\cdot584 \quad \text{body}\\
&3\cdot033 \quad \text{tail}
\end{aligned}
$$

Mean . . . 2·949

From these data, I find by equation (6),—

$$\left.\begin{aligned}
\frac{1}{a} &= 1\\[4pt]
\frac{1}{b} &= 1\\[4pt]
\frac{1}{c} &= 3\cdot697
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathbf{F})$$

EXAMPLE 7.—*Silurian Black Slate of Garth, opposite Port-madoc, North Wales.*

The fossil used in this example was a tail of the *Ogygia scutator*, whose normal dimensions were found to be as follows :—

$$\text{Ogygia scutator} \quad \text{Breadth of tail} = 355$$
$$\text{Length of tail} = 203$$

From these measurements, compared with the distorted fossils, we find—

$$\phi = 11°, \quad \frac{1}{\rho} = 2\cdot335 ;$$

from which we obtain, by the aid of equation (6), the following results :—

$$\left. \begin{array}{l} \dfrac{1}{a} = 1 \\[2mm] \dfrac{1}{b} = 1 \\[2mm] \dfrac{1}{c} = 11\cdot105 \end{array} \right\} \quad . \quad . \quad . \quad . \quad (G)$$

This result corresponds to a greater amount of compression than I found in any other case, not even excepting Tintagel, where the cleavage is uncommonly well developed.

EXAMPLE 8.—*Carboniferous Slates of Carrigaline, Co. Cork.*

The carboniferous slates of this district are of the same geological age as the slates of Ardoginna, Co. Waterford, and of the same mineral character, being dark and carbonaceous at intervals, with patches of smooth brown shale between the dark mud bands. The fossil used to determine the amount of compression is the *Orthis crenistria*, whose breadth equals its length in the normal condition. The measurements of the distorted fossil are as follow :—

$$\text{Orthis crenistria} \quad \text{Length} = 193$$
$$\text{Breadth} = 409$$

From the specimen, on which these measurements were taken, I obtained the following data :—

$$\phi = 80°, \quad \frac{1}{\rho} = 2\cdot119 ;$$

from which, by means of equation (6), I find—

$$\left.\begin{array}{l} \dfrac{1}{a} = 1 \\[2ex] \dfrac{1}{b} = 1 \\[2ex] \dfrac{1}{c} = 2\cdot144 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(H)}$$

This result indicates an amount of compression quite comparable with that observed in the Ardoginna and Ardmore slates of the same age, and may be considered as characteristic of the carboniferous slates of the entire south of Ireland.

It now remains to state how far the results I have obtained correspond with those arrived at by Mr. Sharpe.

At the 87th page of the Quarterly Journal of the Geological Society of London, Mr. Sharpe sums up the results of his observations as follows :—

1st. *In cleaved rocks there is a compression of the mass in a direction everywhere perpendicular to the planes of cleavage.*

2nd. *An expansion of the mass in the direction of the dip of the cleavage.*

3rd. *No proof has been found that the rocks have suffered any change in the direction of the strike of the cleavage planes.*

Mr. Sharpe also mentions incidentally in the course of his paper the following fact, of the importance of which he does not seem to have been aware :—

4th. *When the plane of bedding is considered, the shells all appear contracted in a direction perpendicular to the strike of the cleavage plane across the bed.*

On comparing these results with the laws of cleavage distortion given in the preceding part of this paper, it will be observed that the first and fourth fall in with Laws III. and I. The second and third statements do not accord with my observations, and I am inclined to think that they are hasty generalizations, as it would be extremely difficult to ascertain the *absolute* compression or expansion undergone by the rock in any direction.

It is evident that the whole subject of cleavage and its mechanical origin is very imperfectly understood, and a desire to throw even a little light upon so interesting a subject must be my excuse for offering so imperfect a statement of the numerical results which may be deduced from the measurement of the proportions of the distorted fossils which are found abundantly in many highly cleaved rocks.

It may be worth while to sum up the preceding results in the form of a table, so as to admit of their easy comparison with each other.

Table showing the Relative Compression of Cleaved Rocks.

| Relative compression. | Ardoginna, Waterford. | South Pedhe win, Cornwall. | Tintagel, Cornwall. | Abereiddy, Pemb. oke. | Llyn Padarn, Carnarvon. | Moel Benddu, Merionedh. | Garth, Meriozeth. | Carrigaline, Cork. |
|---|---|---|---|---|---|---|---|---|
| Intersection of cleavage and bedding ......... | 1·000 | 1·000 | 1·000 | 1·000 | 1·000 | 1·000 | 1·000 | 1·000 |
| Dip of cleavage ............ | 1·026 | 0·990 | 1·494 | 1·000 | 1·195 | 1·000 | 1·000 | 1·000 |
| Perpendicular to cleavage ..................... | 2·426 | 3·889 | 9·839 | 6·881 | 1·881 | 3·697 | 11·105 | 2·144 |

The preceding Table is, I believe, the first attempt to estimate numerically the amount of compression necessary to produce cleavage in rocks; and it is to be hoped that the attention of geological observers may in future be more directed to the observation of the relative position of the planes of cleavage and bedding, and of the distortion of the fossils : such observations, to be of any value, should be made in the field, by persons competent to judge of the shape of the fossil forms, and carefully instructed as to the precise points necessary to be observed.

An opportunity of obtaining the information requisite for a perfect mechanical theory of cleavage is afforded by the means of observation possessed by the Staff of the Geological Survey of Great Britain and Ireland. If the operations of such a survey were conducted with the science and skill necessary for carrying out such an important work, nothing would be wanting to complete the data requisite for the solution of many interesting problems in physical geology. But it is much to be feared, that so long as geologists remain in the half-educated condition with which they are at present contented, that such an amount of intelligent observation is not to be looked for; and that those who wish for satisfactory foundations of facts on which to build their theories, must even be content to take their hammers in their hands, and having strapped on their knapsacks, to seek in the field of nature the facts for themselves.

Trinity College, Dublin,
October 20, 1856.

LIII. *Remarks on* Professor Challis's *paper, entitled " A Theory of the Composition of Colours,"* &c. *By* Professor G. G. Stokes [*].

MY object in the present communication is not to discuss Professor Challis's theory, but to rectify some statements as to the experimental facts of the case, as well as one relating

* Communicated by the Author.

to the extent of some researches of my own. I have, however, on some points expressed opinions, respecting the justice of which it is only one who is familiar with certain classes of optical experiments who can feel the confidence that I entertain.

From the paragraph commencing at the foot of page 330, it is plain that Professor Challis has made some confusion between three perfectly distinct things : Sir David Brewster's controverted analysis of the solar spectrum by means of absorbing media[*]; his discovery of the phænomenon of internal dispersion[†]; and my own discovery, that a beam of rays of prismatic purity (whether belonging to the visible or invisible portion of the spectrum is indifferent) may, by their action on certain media, produce light which may be decomposed by the prism into portions extending over a wide range of refrangibility, and having colours answering to their refrangibilities[‡].

As to the first, it was asserted by Sir David Brewster that light of prismatic purity may have its colour changed by passing through absorbing media. This has nothing to do with "internal" or "epipolic" dispersion, or "fluorescence." Glass coloured blue by cobalt, for instance, has none of these properties, although it is one of the media which exhibit most strikingly the phænomena adduced by Sir David Brewster. Were such a change of colour made out, it would be a point of the utmost importance to consider in reference to any physical theory of light. But while none deny that the appearances are as stated by Sir David Brewster, the inference to be drawn from those appearances remains open to discussion. Airy[§], Helmholtz[||], and Bernard[¶], by operating in a different manner, have come to the conclusion that the colour is not changed; and Helmholtz has attributed the apparent change partly to the mixture of a very small quantity of stray light, partly to the effects of contrast. Having been much in the habit of analysing the light transmitted by coloured solutions, and having repeatedly seen the phænomena on which Sir David Brewster relies, I may be permitted to express my belief that the change of colour is only apparent, being an illusion depending upon contrast, and that this is one of the cases in which the direct evidence of the senses must be controlled. Were the change of colour real, Prof. Challis's statement (p. 330), that "experiment has proved that both the colour and the angle of refraction for a given angle of incidence depend,

    [*] Edinburgh Transactions, vol. xii. p. 123.
    [†] Edinb. Trans. vol. xvi. p. 111 ; and Phil. Mag. vol. xxxii. (1848) p. 401.
    [‡] Philosophical Transactions for 1852, p. 463.
    [§] Phil. Mag. vol. xxx. p. 73.
    [||] Poggendorff's *Annalen*, vol. lxxxvi. p. 501.
    [¶] Report of the Meeting of the British Association at Liverpool in 1854, 2nd part, p. 5.

the substance being given, only on the value of λ," would cease to be true.

As to the second, the principal phænomenon consists in this: that when a beam of sunlight, condensed by a lens, is admitted into certain perfectly clear (*i. e.* not muddy) media, the path of the rays is marked by light, of different colours in different cases, which emanates in all directions. As the real nature of this remarkable phænomenon was not at the time understood, and the phænomenon itself was confounded with the effects of mere suspended particles, it is needless to discuss its possible bearing on any theory of the sensation of colour under this head.

As to the third, the new light emanating from the media which possess the property in question is just like any other light of the same prismatic composition. In its physical properties it retains no traces of its parentage, and its colour depends simply upon its new refrangibility, having nothing to do with that of the producing rays, nor to the circumstance of their belonging to the visible or the invisible part of the spectrum. Hence, in speculating on the sensation of colour, this phænomenon may be set aside as not bearing upon the question. I may remark, however, that with regard to the sensation of colour, an analogy has often struck me between the retina and a fluorescent substance, or rather a mixture of three or more fluorescent substances: but this is only an analogy.

It is not true, as Professor Challis seems to suppose (p. 332), that absorption is always, or even generally, accompanied by epipolic dispersion. Among the great variety of coloured metallic solutions, I have hitherto found that property only in solutions of salts of sesquioxide of uranium. I make this remark merely by the way, to prevent misconception: I perfectly agree with Professor Challis in believing that a ray of definite refrangibility is uncompounded; in fact, it was my firm belief in that doctrine which led me to make out the phænomenon of the change of refrangibility of light.

The superposition of two coloured glasses or ribbons by no means gives the effect of the mixture of the two colours. Various methods of mixing colours are enumerated by Mr. Maxwell at the end of his paper, entitled "Experiments on Colour," &c., in the twenty-first volume of the Edinburgh Transactions, p. 275. The production of white by a mixture of blue and yellow is by no means confined to prismatic blue and yellow, but takes place just as well with the colours of coloured bodies. In making experiments with the spectrum, in order to neutralize, when possible, a prismatic colour of given intensity by another prismatic colour, so as to produce white, *two* points must be attended to: the place of the second colour in the spectrum must be properly

chosen, and the intensity of the light properly regulated. Hence any speculations as to the cause of the variations of intensity in the solar spectrum can have no bearing on the subject before us, seeing that the relation between the intensities of the mixed colours necessary for the production of whiteness is a matter of experimental adjustment.

The reason why the superposition of two coloured bodies does not give the mixture of the colours is known, and is very simple. The composition of the light transmitted through a coloured glass may very conveniently be represented by a curve, in the manner of Sir John Herschel, in which the abscissa $x$ denotes refrangibility, measured, suppose, by the distance from the extreme red in some standard spectrum, and the ordinate $y$ denotes the intensity; so that $y\,dx$ is the quantity of light between the refrangibilities $x$ and $x + dx$, the intensity in the incident light being taken equal to unity, for simplicity's sake, whatever be the value of $x$, as we only care to compare intensities for the same value of $x$. Let $y$, $y'$ be the ordinates in the curves belonging to two glasses, $y_s$ the ordinate belonging to the tint obtained by superposing the glasses, $y_m$ the ordinate belonging to the mixed tint, as procured, for instance, by a double-image prism, in which case each of the superposed differently coloured images has half the brightness of the original. Then $y_s = y y'$, but $y_m = \frac{1}{2}(y + y')$; and it is easy to see how different may be the curves whose ordinates are $y_s$, $y_m$ respectively. Thus, let the scale of abscissæ be such that the spectrum extends from $x = 0$ to $x = \pi$, and let $y = \frac{1}{4}(1 - \cos x)^2$, $y' = \frac{1}{4}(1 + \cos x)^2$. In this case $y_s = \frac{1}{16}\sin^4 x$, which vanishes at the extremities, and is a maximum in the middle; whereas $y_m = \frac{1}{4}(1 + \cos^2 x)$, which is a maximum at the two extremities, and a minimum in the middle. In the former case, the tint would be a sort of green, a pretty full colour; in the latter, a sort of dilute purple. The colours of two ribbons may very conveniently be mixed in equal proportion by placing them side by side, and viewing them through a double-image achromatic prism; and it will be seen how different the mixed colour is from that seen on superposing the ribbons and holding them up to the light.

I cannot agree with Professor Challis, that "the coloured light of substances, though derived from sunlight, is in fact new light," except so far as relates to that portion which arises from fluorescence. But fluorescence is often absent altogether; and even when it exists, the colour thence arising must in most cases be but a small fraction of the whole colour observed when the substance is freely exposed to white light, not viewed under absorbing media. I think that any one who has been in the constant habit of analysing by the prism the light transmitted through

clear coloured fluids or solids, and the light transmitted through or reflected from dyed or other coloured substances, must be forced to admit, that, setting aside a comparatively small number of cases in which the colour observed is referrible to other causes, the colours of natural bodies are due to absorption. The exceptions are colours due to fluorescence, as in the case of solutions of quinine, or to regular chromatic reflexion, as in the case of gold, copper, platino-cyanide of magnesium, murexide, &c., not to mention such colours as those of the rainbow, &c., which result from the general properties of bodies with regard to their action on light, not from any speciality of the substance by which the colours happen to be produced. The mode in which I conceive absorption to operate in occasioning the colours observed in dyed ribbons, flowers, coloured powders, &c., I have more fully explained elsewhere\*. Now absorption is best studied in clear solids or solutions, where it is not complicated by irregular reflexions or refractions. But when such media are studied by the aid of a pure spectrum, there cannot be a moment's hesitation that the colour of the transmitted light is due to the abstraction from the incident white light of some of the component rays, as explained by Newton. The colour results, not from the light acted on by the medium, but precisely from the portion left unaffected. Hence its origin is celestial (supposing the sun to be the source of the light employed), not terrestrial. But if the colours of natural bodies arise from absorption, the origin of those colours must be deemed celestial too. To make the origin of the green colour of a leaf terrestrial, but that of the green colour of the light transmitted through an alcoholic solution of the colouring matter celestial, notwithstanding that the two greens agree in their very remarkable prismatic composition, would be needlessly and most capriciously to multiply the causes of natural phænomena. The light which gives us the sensation of greenness when we look at a leaf is, I conceive, no more terrestrial in its origin than the sun's light reflected from a mirror is terrestrial, as not retaining the direction which it had in travelling to us from the sun. It is only in the phænomenon of fluorescence, and the closely allied phænomenon of phosphorescence, that the light emitted can be considered as new light having a terrestrial origin.

---

\* Philosophical Transactions for 1852, p. 527.

LIV. *On the Application of the Mechanical Theory of Heat to the Steam-engine. By* R. CLAUSIUS.

[Concluded from p. 354.]

46. WHEN the tension series for the vapour of any liquid is known with sufficient accuracy, the values of the magnitudes $g$ and $Tg$ for the several temperatures can be calculated from it, and arranged in tables in the same manner as is usually done with the values of $p$.

In the case of steam, hitherto almost solely used for machines, and for the interval of temperature extending from 40° to 200° C., between which the application takes place, I have, with the help of Regnault's tension series, made such a calculation.

Strictly, I ought to have differentiated according to $t$ the formulæ which Regnault used in calculating the several values of $p$ below and above 100° C., and then to have calculated $g$ by means of the new formulæ thus obtained. But as it appeared to me that those formulæ did not fulfil their purpose perfectly enough to justify so large an amount of labour, and as the calculation and institution of another suitable formula would have been still more tedious, I contented myself with using the numbers already calculated for the pressure in order approximately to determine the differential coefficient of the pressure. For example, $p_{146}$ and $p_{148}$ being the pressures for the temperatures 146° and 148°, I have assumed that the magnitude

$$\frac{p_{148} - p_{146}}{2}$$

represents with sufficient accuracy the value of the differential coefficient for the mean temperature 147°.

In doing this, I have for temperatures above 100° used the numbers given by Regnault himself*. With respect to the values below 100°, Moritz† has lately drawn attention to the fact that the formula employed by Regnault between 0° and 100° was, especially in the vicinity of 100°, somewhat incorrect in consequence of his having used logarithms of seven places in calculating the constants. In consequence of this, Moritz has calculated those constants with logarithms of ten places, basing his calculations on the same observed values; and he has published the values of $p$—as far as they differ from Regnault's, which only occurs above 40°,—thus deduced from the corrected formulæ. I have used these values.

As soon as $g$ is calculated for the several temperatures, the

* *Mém. de l'Acad. des Sciences,* vol. xiii. p. 625.
† *Bulletin de la Classe Physico-mathématique de l'Acad. de St. Pétersbourg,* vol. xiii. p. 41.

calculation of $T . g$ also is attended with no further difficulty, because T is determined from the simple equation

$$T = 273 + t.$$

I have collected the values of $g$ and $T . g$ and given them in a Table at the end of this memoir. For the sake of completeness, I have also added the corresponding values of $p$; those above 100° being calculated by Regnault, those below by Moritz. To each of these three series of numbers are attached the differences between every two successive numbers; so that from the Table the values of the three magnitudes can be found for every temperature; and conversely, for any given value of one of the three magnitudes the corresponding temperature can be seen.

After what was before said of the calculation of $g$, it need scarcely be mentioned that the numbers of this table are not to be considered as exact, they being only communicated in the absence of better ones. As, however, the calculations with reference to steam-engines are always based upon rather uncertain data, the numbers can without hesitation be used for this purpose, there being no fear that the uncertainty of the result will be much increased thereby.

As to the method of application, however, another remark is still necessary. In the equations (XVII), it is assumed that the pressure $p$ and its differential coefficient $g$ are expressed in kilogrammes to a square metre; whereas in the table the same unit of pressure, a millimetre of mercury, is retained as that referred to in Regnault's tension series. In order, notwithstanding this, to be able to apply the table, it is only necessary to divide every term in those equations, which does not contain either $p$ or $q$ as factor, by the number 13·596. This number, which is nothing more than the specific gravity of mercury at 0° C., compared with water at its maximum density, will for the sake of brevity be represented by $k$.

This change of the formulæ, however, scarcely increases the calculation, inasmuch as it is equivalent to substituting everywhere, in place of the constant factor $\frac{1}{A}$,—which, according to Joule, has the value 423·55 already mentioned,—the other constant,

$$\frac{1}{Ak} = \frac{423·55}{13·596} = 31·1525 ; \quad . \quad . \quad . \quad . \quad (46)$$

when, instead of the work W, the magnitude $\frac{W}{k}$ will be found in the first instance, and will subsequently merely have to be multiplied by $k$.

47. Let us now return to the equations (XVII), and consider first the second of them.

This equation may be written in the following form :—

$$T_2 g_2 = C + a(t_1 - t_2) - b(p_1 - p_2), \quad . \quad . \quad . \quad (47)$$

wherein the magnitudes C, $a$, and $b$ are independent of $t_2$, and have the following values :—

$$\left. \begin{array}{l} C = \dfrac{1}{e V - l \sigma}\left[\dfrac{r_1}{A k} + \epsilon V \left(\dfrac{r_0 - c(T_1 - T_0)}{A k u_0} + p_1 - p_0\right)\right] \\[3mm] a = \dfrac{c\left(l + \dfrac{\epsilon V}{u_0}\right)}{A k (e V - l \sigma)} \\[3mm] b = \dfrac{\epsilon V - l \sigma}{e V - l \sigma} \end{array} \right\} . \quad (47a)$$

Of the three terms on the right-hand side of (47), the first far exceeds the others ; hence it will be possible, by successive approximation, to determine the product $T_2 g_2$, and thence also the temperature $t_2$.

In order to obtain the first approximate value of the product, which we will call $T' g'$, let us on the right side of (47) set $t_1$ in the place of $t_2$, and correspondingly $p_1$ in place of $p_2$, then

$$T' g' = C. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

The temperature $t'$, corresponding to this value of the product, can be sought in the table. In order to find a second approximate value of the product, the value of $t'$ just found, and the corresponding value of the pressure $p'$, are introduced into (47) in the places of $t_2$ and $p_2$, whereby, having regard to the former equation, we have

$$T'' g'' = T' g' + a(t_1 + t') - b(p_1 - p'). \quad . \quad . \quad (48a)$$

As before, the temperature $t''$, corresponding to this value of the product, is given by the table. If this does not with sufficient exactitude represent the required temperature $t_2$, the same method must be repeated. The newly-found values $t''$ and $p''$ must be substituted in (47) in place of $t_2$ and $p_2$, whereby with the assistance of the two last equations, we have

$$T''' g''' = T'' g'' + a(t' - t'') - b(p' - p''), \quad . \quad . \quad (48b)$$

and in the table we can find the new temperature $t'''$.

We might proceed in this manner for any length of time, though we shall find that the third approximation is already within $\frac{1}{100}$th, and the fourth within $\frac{1}{1000}$th of a degree of the true value of the temperature $t_2$.

48. The treatment of the third of the equations (XVII) is precisely similar. If we divide by $V - l\sigma$, and for facility of cal-

culation introduce Briggs's logarithms (Log) in place of natural logarithms (log) by dividing by M the modulus of this system, the equation will take the form

$$g_3 = C + a \operatorname{Log} \frac{T_2}{T_3}, \quad \ldots \quad \ldots \quad (49)$$

wherein C and $a$ are independent of $T_3$, and have the following values :—

$$\left. \begin{array}{c} C = \dfrac{eV - l\sigma}{V - l\sigma} \cdot g_2 \\[2mm] a = \dfrac{c\left(l + \dfrac{\epsilon V}{u_0}\right)}{M \cdot Ak(V - l\sigma)} \end{array} \right\} \quad \ldots \quad (49a)$$

Again, in equation (49) the first term on the right is greatest, so that we can apply the method of successive approximation. In the first place, $T_2$ is put in the place of $T_3$, and we obtain the first approximate value of $g_3$, viz.

$$g' = C, \quad \ldots \quad \ldots \quad \ldots \quad (50)$$

from which we can find the corresponding temperature $t'$ in the tables, and thence the absolute temperature $T'$. This is now substituted for $T_3$ in (49), and gives

$$g'' = g' + a \operatorname{Log} \frac{T_2}{T'}, \quad \ldots \quad \ldots \quad (50a)$$

whence $T''$ is found. Similarly we obtain

$$g''' = g'' + a \operatorname{Log} \frac{T'}{T''}, \quad \ldots \quad \ldots \quad (50b)$$

and so forth.

49. Before proceeding to the numerical application of the equations (XVII), the magnitudes $c$ and $r$ alone remain to be determined.

The magnitude $c$, which is the specific heat of the liquid, has hitherto been treated as constant in our development. Of course this is not quite correct, for the specific heat increases a little with increasing temperature. If, however, we select as a common value the one which is correct for about the middle of the interval over which the temperatures involved in the investigation extend, the deviations cannot be important ones. In machines driven by steam, this mean temperature may be taken at $100°$ C.; this being, in ordinary high-pressure engines, about equally distant from the temperature of the boiler and that of the condenser. In the case of water, therefore, we will employ the

number which, according to Regnault, expresses its specific heat at 100°, and thus set

$$c = 1\cdot0130. \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (51)$$

In the determination of $r$ we shall start from the equation

$$\lambda = 606\cdot5 + 0\cdot305 \cdot t,$$

given by Regnault as expressing the whole quantity of heat necessary to raise the unit of weight of water from 0° to the temperature $t$, and afterwards to evaporate it at that temperature. According to this definition, however,

$$\lambda = \int_0^t c\,dt + r,$$

so that

$$r = 606\cdot5 + 0\cdot305 \cdot t - \int_0^t c\,dt.$$

In order to obtain precisely Regnault's value of $r$, we ought to substitute for $c$ in the above integral, the function of the temperature which Regnault determined. For our present purpose, however, I think it will suffice to give to $c$ the constant above selected, by means of which

$$\int_0^t c\,dt = 1\cdot013 \cdot t,$$

and the two terms in the above equation involving $t$ combine to form the single one $-0\cdot708 \cdot t$.

At the same time we must alter the constant term of the equation a little, and determine it so that the formula will correctly express that observed value of $r$ which in all probability is most accurate. As a mean of thirty-eight observations, Regnault found the value of $\lambda$ at 100° to be 636·67. Deducting the quantity of heat necessary to raise the unit of weight of water from 0° to 100°, which, according to Regnault, amounts to 100·5 units of heat, and contenting ourselves with one decimal, there remains

$$r_{100} = 536\cdot2 *.$$

Employing this value, we obtain the following formula:—

$$r = 607 - 0\cdot708 \cdot t. \quad \ldots \quad \ldots \quad \ldots \quad (52)$$

The following comparison of a few values calculated herefrom, with the corresponding ones given by Regnault in his tables†,

---

* In his tables Regnault gives, instead of this, the number 536·5; the reason is, however, that instead of the above value 636·67 for $\lambda$ at 100°, he used the round number 637 in his calculations.

† *Mém. de l'Acad. des Sciences*, vol. xxi. p. 748.

will show that this simplified formula agrees sufficiently well with the more accurate method of calculation above alluded to :—

| *t.* | 0°. | 50°. | 100°. | 150°. | 200°. |
|---|---|---|---|---|---|
| *r* according to equation (52) | 607·0 | 571·6 | 536·2 | 500·8 | 465·4 |
| *r* according to Regnault ... | 606·5 | 571·6 | 536·5 | 500·7 | 464·3 |

50. In order to be able to distinguish between the effects of the two different kinds of expansions to which the two last of the equations (XVII) refer, it will perhaps be best to consider in the first place a steam-engine in which only one of them takes place. We will commence, therefore, with one of the machines which are said to work *without expansion.*

In this case $e$, which expresses the relation of the volumes before and after expansion, equals 1, and at the same time $T_3 = T_2$; so that the equations (XVII) assume a simpler form.

The last of these equations becomes identical, and therefore vanishes. Further, many terms of the first will admit of elimination, because they now become like the corresponding terms of the second, from which they before differed only by containing $T_3$ instead of $T_2$. Introducing the above-mentioned quantity $k$ at the same time, we now obtain

$$\left. \begin{aligned} \frac{W}{k} &= V(1-\epsilon)(p_2-p_0) - l\sigma(p_1-p_0) \\ (V-l\sigma)T_2 g_2 &= \frac{r_1 + lc(T_1-T_2)}{Ak} + \epsilon V \cdot \left( \frac{r_0 - c(T_2-T_0)}{Aku_0} + p_2-p_0 \right) \\ &\quad + l\sigma(p_1-p_2) \end{aligned} \right\} \text{(XVIII)}$$

The first of these two equations is exactly the same as the one which we also obtain by Pambour's theory, if in (XII) we make $e = 1$, and introduce V instead of B. The difference therefore consists merely in the second equation, which takes the place of the simple relation between volume and weight assumed by Pambour.

51. To the quantity $\epsilon$, which occurs in these equations and represents the vicious space as a fractional part of the whole space set free to the vapour, we will give the value 0·05. The quantity of liquid which the vapour carries with it on entering the cylinder varies in different machines. Pambour states that it amounts on the average to 0·25 in locomotives, but in stationary engines to much less, probably only to 0·05 of the whole mass entering the cylinder. In our example we will make use of the latter number, according to which the ratio of the whole mass entering the cylinder is to the vaporous part of the same as

$1 : 0.95$. Further, let the pressure in the boiler be five atmospheres, to which the temperature $152°.22$ belongs, and let us suppose that the machine has no condenser, or, in other words, let it have a condenser with the pressure of one atmosphere. The mean reaction in the cylinder is accordingly greater than one atmosphere. As before mentioned, the difference in locomotives may be considerable, but in stationary engines it is smaller. With respect to stationary engines, Pambour has altogether neglected this difference; and as our only object at present is to compare the new formulæ with those of Pambour, we will also disregard the difference, and let $p_0 = 1$ atmosphere.

In this example, therefore, the following values will have to be made use of in equations (XVIII):—

$$\left.\begin{array}{l} \epsilon = 0.05 \\ l = \dfrac{1}{0.95} = 1.053 \\ p_1 = 3800 \\ p_0 = 760 \end{array}\right\} \quad \ldots \quad (53)$$

To these must be added the values

$$k = 13.596$$
$$\sigma = 0.001,$$

which are the same for all cases; and then in the first of the equations (XVIII), besides the required value of W, the magnitudes V and $p_2$ alone will remain undetermined.

52. We must now examine the *least possible* value of V.

This value corresponds to the case where the pressure in the cylinder is the same as that in the boiler, so that we have merely to put $p_1$ in the place of $p_2$ in the last of equations (XVIII) in order to obtain

$$V = \frac{\dfrac{r_1}{Ak} + l\sigma \cdot T_1 g_1}{T_1 g_1 - \epsilon \left( \dfrac{r_0 - c(T_1 - T_0)}{Aku_0} + p_1 - p_0 \right)}. \quad \ldots \quad (54)$$

In order at once to give an example of the influence of the vicious space, I have calculated two values of this expression, corresponding respectively to the cases where no vicious space exists ($\epsilon = 0$), and where, according to supposition, $\epsilon = 0.05$. These values, expressed as fractions of a cubic metre to one kilogramme of vapour from the boiler, are

$$0.3637 \text{ and } 0.3690.$$

The latter value is greater than the former, because, first, the

vapour entering the vicious space with greater velocity, the *vis viva* of its motion is converted into heat, which in its turn causes the evaporation of the accompanying liquid; and secondly, because the vapour before present in the vicious space contributes to the increase of the ultimate quantity of vapour.

Substituting both the above values of V in the first of equations (XVIII), and in the one case again making $\epsilon = 0$, whilst in the other $\epsilon = 0.05$, we have as the corresponding quantities of work expressed in kilogramme-metres, the numbers

<div align="center">14990 and 14450.</div>

According to Pambour's theory, it makes no difference whether a part of the volume is vicious space or not; in both cases this volume is determined from the equation (29b) by giving to $p$ the particular value $p_1$. By so doing we obtain

<div align="center">0.3883.</div>

This value is greater than the one (0.3637) before found for the same quantity of vapour, because hitherto the volume of vapour at its maximum density was esteemed greater than, according to the mechanical theory of heat, it can be, and this former estimate also finds expression in equation (29b).

If, by means of this volume, we determine the work under the two suppositions $\epsilon = 0$ and $\epsilon = 0.05$, we have

<div align="center">16000 and 15200.</div>

As might have been concluded immediately from the greater volume, these quantities of work are both greater than those before found, but not in the same ratio; for, according to our equations, the loss of work occasioned by vicious space is less than it would be according to Pambour's theory.

53. In a machine of the kind here considered, which Pambour actually examined, the velocity which the machine actually possessed, compared with the minimum velocity calculated, according to his theory, for the same intensity of evaporation and the same pressure in the boiler, gave the ratio $1.275 : 1$ in one experiment, and in another, where the charge was less, $1.70 : 1$. These velocities would in our case correspond to the volumes 0.495 and 0.660. As an example of the determination of work, we will now choose a velocity between these two, and set simply,

<div align="center">$V = 0.6$.</div>

In order next to find the temperature $t_2$ corresponding to this value of V, we employ the equation (47) under the following special form :—

$$T_2 g_2 = 26577 + 56.42 \cdot (t_1 - t_2) - 0.0483 \cdot (p_1 - p_2). \quad (55)$$

Effecting, by means of this equation, the successive determinations of $t_2$ described in § 47, we obtain the following series of approximate values:—

$$t' = 13\overset{\circ}{3}\cdot01$$
$$t'' = 134\cdot43$$
$$t''' = 134\cdot32$$
$$t'''' = 134\cdot33$$

Further approximate values would only differ from each other in higher decimal places; so that, contenting ourselves with two decimal places, the last number may be considered as the true value of $t_2$.   The corresponding pressure is

$$p_2 = 2308\cdot30.$$

Applying these values of V and $p_2$, as well as those given in § 51, to the first of the equations (XVIII), we obtain

$$W = 11960.$$

Pambour's equation (XII) gives for the same volume 0·6, the work

$$W = 12520.$$

54. In order to show more clearly the dependence of the work upon the volume, and at the same time the difference which exists between Pambour's and my own theory in this respect, I have made a calculation, similar to the last, for a series of other volumes increasing uniformly.   The results are comprised in the following Table.   The first horizontal row of numbers, separated from the rest by a line, contains the values found for a machine without vicious space.   In other respects the arrangement of the Table will be easily understood.

| V. | $t_2$. | W. | According to Pambour. | |
|---|---|---|---|---|
| | | | V. | W. |
| 0·3637 | 15$\overset{2}{2}$·22 | 14990 | 0·3883 | 16000 |
| 0·3690 | 152·22 | 14450 | 0·3883 | 15200 |
| 0·4 | 149·12 | 14100 | 0·4 | 15050 |
| 0·5 | 140·83 | 13020 | 0·5 | 13780 |
| 0·6 | 134·33 | 11960 | 0·6 | 12520 |
| 0·7 | 129·03 | 10910 | 0·7 | 11250 |
| 0·8 | 124·55 | 9880 | 0·8 | 9980 |
| 0·9 | 120·72 | 8860 | 0·9 | 8710 |
| 1 | 117·36 | 7840 | 1 | 7440 |

We see that the quantities of work calculated according to Pambour's theory diminish more quickly with increasing volume

than those calculated from our equations; for at first the former are considerably greater than the latter, afterwards they approach a little nearer, and finally they are absolutely less. The reason is, that, according to Pambour's theory, the same mass, as at first, always remains vaporous during expansion; whilst, according to our theory, a part of the liquid accompanying the vaporous mass afterwards evaporates, and the more so the greater the expansion.

55. In a similar manner we will now consider a machine which works with expansion, and we will further select one with a condenser.

With reference to the magnitude of the expansion, we will suppose that the cylinder is cut off from the boiler when the piston has completed one-third of its journey. Then for the determination of $e$ we have the equation

$$e - \epsilon = \frac{1}{3}(1 - \epsilon);$$

whence, retaining the former value, 0·05, of $\epsilon$,

$$e = \frac{1 \cdot 1}{3} = 0 \cdot 3666 \ldots$$

As before, let the pressure in the boiler be five atmospheres. By good arrangement the pressure in the condenser may be kept below one-tenth of an atmosphere. As it is not always so small, however, and as the reacting pressure in the cylinder always exceeds it a little, we will assume the mean reaction to be one-fifth of an atmosphere (or 152 millims.), to which the temperature $t_0 = 60° \cdot 46$ corresponds. Retaining the former assumed value of $l$, therefore, the quantities requiring application in this example are

$$\left.\begin{array}{l} e = 0 \cdot 36667 \\ \epsilon = 0 \cdot 05 \\ l = 1 \cdot 053 \\ p_1 = 3800 \\ p_0 = 152 \end{array}\right\} \quad \cdots \quad \cdots \quad (56)$$

In order to calculate the work, we now only require the value of V to be given. To guide our choice, we must first know the least possible value of V, which we can find, as before, from the second of the equations (XVII) by putting $p_1$ in the place of $p_2$, and changing the other quantities dependent on $p$ accordingly. In this manner we find for the present case the value

$$1 \cdot 010.$$

Starting from this, we will assume, as a first example, that the actual velocity of the machine's motion has to this minimum a

ratio of 3 : 2 nearly ; so that setting

$$V = 1 \cdot 5,$$

we will determine the work for this velocity.

56. The temperatures $t_2$ and $t_3$ must now be determined by setting this value of V in the two last of equations (XVII). For the machine without a condenser, the determination of $t_2$ has been sufficiently explained ; and as the present case differs from that one only by a different value for $e$, which was there equal to 1, it will be sufficient to state here that the final result is

$$t_2 = 137° \cdot 43.$$

The equation (49), which serves to determine $t_3$, now takes the form

$$g_3 = 26 \cdot 604 + 51 \cdot 515 \operatorname{Log} \frac{T_2}{T_3}, \quad . \quad . \quad . \quad (57)$$

and from it we obtain the following approximate values :—

$$t' = 99 \cdot 24$$
$$t'' = 101 \cdot 93$$
$$t''' = 101 \cdot 74$$
$$t'''' = 101 \cdot 76.$$

We may consider the last of these values, from which the following ones would only differ in higher places of decimals, as the proper value of $t_3$ ; and we may use it, together with the known values of $t_1$ and $t_0$, in the first of the equations (XVII). By so doing we find

$$W = 31080.$$

When, assuming the same value of V, we calculate the work according to Pambour's equation (XII),—whereby, however, the values of B and $b$ are not taken from equation (29$b$), as in the machine without condenser, but from equation (29$a$) intended for machines with condensers,—we find

$$W = 32640.$$

57. In a manner similar to the foregoing I have also calculated the work for the volumes 1·2, 1·8, and 2·1. Besides this, in order to illustrate by an example the influence which the several imperfections have upon the work, I have added the following cases :—

(1) The case of a machine having no vicious space, and where at the same time the pressure in the cylinder during the entrance of the vapour is equal to that in the boiler, and the expansion is carried so far that the pressure diminishes from its original value $p_1$ to $p_0$. If we further suppose that $p_0$ is exactly the pressure

in the condenser, this case will be the one to which equation (XI) refers, and which for a given quantity of heat—the temperatures at which the heat is received and imparted being also considered as given—furnishes the greatest possible quantity of work.

(2) The case of a machine, again, having no vicious space, and when the pressure in the cylinder is again equal to that in the boiler, but where the expansion is not, as before, complete, but only continued until the ratio $e:1$ is obtained. This is the case to which equation (X) refers; only in order to determine the amount of expansion, the change caused by the same in the temperature of the vapour was before supposed to be known, whilst here the expansion is determined according to the volume, and the change of temperature must be afterwards calculated therefrom.

(3) The case of a machine with vicious space and incomplete expansion, and where, of the former favourable conditions, the only one which remains is that during the entrance of the vapour the pressure in the cylinder is the same as in the boiler, so that the volume has its smallest possible value.

To these cases may be added the one already mentioned, where the last favourable condition is relinquished, and the volume has a greater than its minimum value.

For the sake of comparison, all these cases, with the exception of the first, are also calculated according to Pambour's theory. The reason of the exception is, that the equations (29$a$) and (29$b$) do not here suffice; for even the one which is intended for small pressures cannot be applied below one-half, or at most one-third of an atmosphere, whereas here the pressure ought to decrease to one-fifth of an atmosphere.

The following are the numbers given by our equations in the first of the above cases :—

| Volume before expansion. | Volume after expansion. | W. |
|---|---|---|
| 0·3637 | 6·345 | 50460 |

For all the rest of the above cases the results are given in the following Table, where the numbers referring to a machine without vicious space, are again separated from the rest by a horizontal line. The volumes after expansion are alone given, because the corresponding ones before expansion, being in all cases smaller in the proportion of $e:1$, may be easily found :—

| V. | $t_2$. | $t_3$. | W. | According to Pambour. | |
|---|---|---|---|---|---|
| | | | | V. | W. |
| 0·992 | 152·22 | 113·71 | 34300 | 1·032 | 36650 |
| 1·010 | 152·22 | 113·68 | 32430 | 1·032 | 34090 |
| 1·2 | 145·63 | 108·38 | 31870 | 1·2 | 33570 |
| 1·5 | 137·43 | 101·76 | 31080 | 1·5 | 32640 |
| 1·8 | 131·02 | 96·55 | 30280 | 1·8 | 31710 |
| 2·1 | 125·79 | 92·30 | 29490 | 2·1 | 30780 |

58. The quantities of work in this Table, as well as those in the former Table for machines without condensers, refer to a kilogramme of vapour coming from the boiler. It is easy, however, to refer the work to a *unit of heat* furnished by the source of heat; for every kilogramme of vapour requires as much heat as is necessary, first to raise the mass $l$ (somewhat more than one kilogramme) from the temperature it had when entering the boiler up to the general temperature of the same, and then at that temperature to convert a kilogramme of it into vapour. This quantity of heat can be calculated from former data.

59. In conclusion, I will add a few remarks on *friction*, restricting myself, however, to a justification of my having hitherto disregarded friction in the developed equations, by showing that instead of introducing the same at once into the first general expressions for the work, as Pambour has done, it may also, according to the same principles, and according to the manner of other authors, be afterwards brought into calculation.

The forces which the machine has to overcome during its action may be thus distinguished:—(1) The resistance externally opposed to it, and in overcoming which it performs the required *useful* work. Pambour calls this resistance the charge of the machine. (2) The resistances which have their source in the machine itself, so that the work expended in overcoming them is not externally of use. All these resistances are included in the term *friction*; although, besides friction in its more limited sense, they comprise other forces, particularly the resistances caused by pumps belonging to the machine, exclusive of the one which feeds the boiler, and which has already been considered.

Pambour brings both these kinds of resistances into calculation as forces opposing the motion of the piston; and in order conveniently to combine them with the pressures of the vapour on both sides of the piston, he also adopts a notation similar to the one ordinarily used for vapour pressures; that is to say, the

symbol denotes, not the whole force, but that part of it which corresponds to a unit of surface of the piston. In this sense let the letter R represent the charge.

A further distinction must still be made in the case of friction, for it has not a constant value in each machine, but increases with the charge. Accordingly Pambour divides it into two parts : that which is already present when the machine moves without charge, and that which the charge itself occasions. With respect to the last, he assumes that it is proportional to the charge. Accordingly, the friction referred to the unit of surface is expressed by

$$f + \delta . R,$$

where $f$ and $\delta$ are magnitudes which, although dependent upon the construction and dimensions of the machine, are, according to Pambour, to be considered as constant in any given machine.

We can now refer the work of the machine to these *resisting* forces instead of, as before, to the *driving* force of steam ; for the negative work done by the former must be equal to the positive work done by the latter, otherwise an acceleration or retardation of motion would ensue, which would be contradictory to the hypothesis of uniform motion hitherto made. During the time that a unit of weight of vapour enters the cylinder, the surface of the piston describes the space $(1-\epsilon)V$, hence for the work W we obtain the expression

$$W = (1-\epsilon)V[(1+\delta) . R + f].$$

On the other hand, the *useful* part of this work, which for distinction from the whole work shall be symbolized by (W), is expressed thus,

$$(W) = (1-\epsilon)V . R.$$

Eliminating R from this equation by means of the former, we have the equation

$$(W) = \frac{W - (1-\epsilon)V . f}{1 + \delta}, \quad . \quad . \quad . \quad (58)$$

by means of which, V being known, the useful work (W) can be deduced from the whole work W as soon as the quantities $f$ and $\delta$ are given.

I will not here enter into Pambour's method of finding the latter quantities, as this determination still rests upon a too insecure basis, and as friction is altogether foreign to the subject of this memoir.

TABLE CONTAINING THE VALUES, FOR STEAM, OF $p$, ITS DIFFERENTIAL COEFFICIENT $\dfrac{dp}{dt} = g$, AND THE PRODUCT $T . g$ EXPRESSED IN MILLIMETRES OF MERCURY.

| $t$ in Centigrade degrees. | $p$. | $\Delta$. | $g$. | $\Delta$. | $T . g$. | $\Delta$. |
|---|---|---|---|---|---|---|
| 40 | 54·906 | 3·003 | 2·935 | 0·139 | 919 | 46 |
| 41 | 57·909 | 3·145 | 3·074 | 0·144 | 965 | 49 |
| 42 | 61·054 | 3·291 | 3·218 | 0·149 | 1014 | 50 |
| 43 | 64·345 | 3·444 | 3·367 | 0·155 | 1064 | 52 |
| 44 | 67·789 | 3·601 | 3·522 | 0·161 | 1116 | 55 |
| 45 | 71·390 | 3·766 | 3·683 | 0·167 | 1171 | 57 |
| 46 | 75·156 | 3·935 | 3·850 | 0·173 | 1228 | 59 |
| 47 | 79·091 | 4·112 | 4·023 | 0·180 | 1287 | 62 |
| 48 | 83·203 | 4·294 | 4·203 | 0·185 | 1349 | 64 |
| 49 | 87·497 | 4·483 | 4·388 | 0·193 | 1413 | 67 |
| 50 | 91·980 | 4·679 | 4·581 | 0·199 | 1480 | 69 |
| 51 | 96·659 | 4·882 | 4·780 | 0·207 | 1549 | 72 |
| 52 | 101·541 | 5·092 | 4·987 | 0·213 | 1621 | 74 |
| 53 | 106·633 | 5·309 | 5·200 | 0·221 | 1695 | 78 |
| 54 | 111·942 | 5·533 | 5·421 | 0·228 | 1773 | 80 |
| 55 | 117·475 | 5·766 | 5·649 | 0·237 | 1853 | 83 |
| 56 | 123·241 | 6·006 | 5·886 | 0·244 | 1936 | 87 |
| 57 | 129·247 | 6·254 | 6·130 | 0·252 | 2023 | 89 |
| 58 | 135·501 | 6·510 | 6·382 | 0·260 | 2112 | 93 |
| 59 | 142·011 | 6·775 | 6·642 | 0·269 | 2205 | 96 |
| 60 | 148·786 | 7·048 | 6·911 | 0·278 | 2301 | 100 |
| 61 | 155·834 | 7·330 | 7·189 | 0·286 | 2401 | 103 |
| 62 | 163·164 | 7·621 | 7·475 | 0·296 | 2504 | 107 |
| 63 | 170·785 | 7·922 | 7·771 | 0·305 | 2611 | 111 |
| 64 | 178·707 | 8·231 | 8·076 | 0·314 | 2722 | 114 |
| 65 | 186·938 | 8·550 | 8·390 | 0·325 | 2836 | 118 |
| 66 | 195·488 | 8·880 | 8·715 | 0·334 | 2954 | 123 |
| 67 | 204·368 | 9·218 | 9·049 | 0·344 | 3077 | 126 |
| 68 | 213·586 | 9·568 | 9·393 | 0·355 | 3203 | 131 |
| 69 | 223·154 | 9·928 | 9·748 | 0·365 | 3334 | 135 |
| 70 | 233·082 | 10·298 | 10·113 | 0·376 | 3469 | 139 |
| 71 | 243·380 | 10·680 | 10·489 | 0·387 | 3608 | 144 |
| 72 | 254·060 | 11·072 | 10·876 | 0·398 | 3752 | 149 |
| 73 | 265·132 | 11·476 | 11·274 | 0·410 | 3901 | 153 |
| 74 | 276·608 | 11·892 | 11·684 | 0·422 | 4054 | 159 |
| 75 | 288·500 | 12·320 | 12·106 | 0·433 | 4213 | 163 |
| 76 | 300·820 | 12·759 | 12·539 | 0·445 | 4376 | 168 |
| 77 | 313·579 | 13·210 | 12·984 | 0·458 | 4544 | 174 |
| 78 | 326·789 | 13·675 | 13·442 | 0·471 | 4718 | 179 |
| 79 | 340·464 | 14·152 | 13·913 | 0·484 | 4897 | 185 |
| 80 | 354·616 | 14·642 | 14·397 | 0·497 | 5082 | 190 |
| 81 | 369·258 | 15·146 | 14·894 | 0·511 | 5272 | 197 |
| 82 | 384·404 | 15·664 | 15·405 | 0·524 | 5469 | 202 |
| 83 | 400·068 | 16·194 | 15·929 | 0·538 | 5671 | 208 |
| 84 | 416·262 | 16·740 | 16·467 | 0·552 | 5879 | 214 |
| 85 | 433·002 | 17·299 | 17·019 | 0·577 | 6093 | 220 |
| 86 | 450·301 | 17·874 | 17·586 | 0·582 | 6313 | 227 |
| 87 | 468·175 | 18·463 | 18·168 | 0·597 | 6540 | 234 |
| 88 | 486·638 | | 18·765 | | 6774 | |

Table (*continued*).

| t in Centigrade degrees. | p. | Δ. | g. | Δ. | T . g. | Δ. |
|---|---|---|---|---|---|---|
| 88 | 486·638 | | 18·765 | | 6774 | |
| | | 19·067 | | 0·612 | | 240 |
| 89 | 505·705 | | 19·377 | | 7014 | |
| | | 19·687 | | 0·628 | | 248 |
| 90 | 525·392 | | 20·005 | | 7262 | |
| | | 20·323 | | 0·644 | | 254 |
| 91 | 545·715 | | 20·649 | | 7516 | |
| | | 20·975 | | 0·660 | | 262 |
| 92 | 566·690 | | 21·309 | | 7778 | |
| | | 21·643 | | 0·676 | | 269 |
| 93 | 588·333 | | 21·985 | | 8047 | |
| | | 22·328 | | 0·694 | | 276 |
| 94 | 610·661 | | 22·679 | | 8323 | |
| | | 23·031 | | 0·712 | | 285 |
| 95 | 633·692 | | 23·391 | | 8608 | |
| | | 23·751 | | 0·728 | | 292 |
| 96 | 657·443 | | 24·119 | | 8900 | |
| | | 24·488 | | 0·747 | | 300 |
| 97 | 681·931 | | 24·865 | | 9200 | |
| | | 25·243 | | 0·765 | | 309 |
| 98 | 707·174 | | 25·630 | | 9509 | |
| | | 26·017 | | 0·783 | | 317 |
| 99 | 733·191 | | 26·413 | | 9826 | |
| | | 26·809 | | 0·787 | | 320 |
| 100 | 760·00 | | 27·200 | | 10146 | |
| | | 27·59 | | 0·805 | | 328 |
| 101 | 787·59 | | 28·005 | | 10474 | |
| | | 28·42 | | 0·840 | | 343 |
| 102 | 816·01 | | 28·845 | | 10817 | |
| | | 29·27 | | 0·855 | | 350 |
| 103 | 845·28 | | 29·700 | | 11167 | |
| | | 30·13 | | 0·865 | | 356 |
| 104 | 875·41 | | 30·565 | | 11523 | |
| | | 31·00 | | 0·885 | | 367 |
| 105 | 906·41 | | 31·450 | | 11888 | |
| | | 31·90 | | 0·915 | | 378 |
| 106 | 938·31 | | 32·365 | | 12266 | |
| | | 32·83 | | 0·935 | | 388 |
| 107 | 971·14 | | 33·300 | | 12654 | |
| | | 33·77 | | 0·955 | | 397 |
| 108 | 1004·91 | | 34·255 | | 13051 | |
| | | 34·74 | | 0·975 | | 407 |
| 109 | 1039·65 | | 35·230 | | 13458 | |
| | | 35·72 | | 0·990 | | 414 |
| 110 | 1075·37 | | 36·220 | | 13872 | |
| | | 36·72 | | 1·010 | | 424 |
| 111 | 1112·09 | | 37·230 | | 14296 | |
| | | 37·74 | | 1·030 | | 434 |
| 112 | 1149·83 | | 38·260 | | 14730 | |
| | | 38·78 | | 1·060 | | 448 |
| 113 | 1188·61 | | 39·320 | | 15178 | |
| | | 39·86 | | 1·080 | | 457 |
| 114 | 1228·47 | | 40·400 | | 15635 | |
| | | 40·94 | | 1·100 | | 467 |
| 115 | 1269·41 | | 41·500 | | 16102 | |
| | | 42·06 | | 1·125 | | 479 |
| 116 | 1311·47 | | 42·625 | | 16581 | |
| | | 43·19 | | 1·150 | | 491 |
| 117 | 1354·66 | | 43·775 | | 17072 | |
| | | 44·36 | | 1·170 | | 502 |
| 118 | 1399·02 | | 44·945 | | 17574 | |
| | | 45·53 | | 1·185 | | 509 |
| 119 | 1444·55 | | 46·130 | | 18083 | |
| | | 46·73 | | 1·220 | | 526 |
| 120 | 1491·28 | | 47·350 | | 18609 | |
| | | 47·97 | | 1·245 | | 537 |
| 121 | 1539·25 | | 48·595 | | 19146 | |
| | | 49·22 | | 1·260 | | 547 |
| 122 | 1588·47 | | 49·855 | | 19693 | |
| | | 50·49 | | 1·290 | | 560 |
| 123 | 1638·96 | | 51·145 | | 20253 | |
| | | 51·80 | | 1·315 | | 574 |
| 124 | 1690·76 | | 52·460 | | 20827 | |
| | | 53·12 | | 1·335 | | 583 |
| 125 | 1743·88 | | 53·795 | | 21410 | |
| | | 54·47 | | 1·365 | | 599 |
| 126 | 1798·35 | | 55·160 | | 22009 | |
| | | 55·85 | | 1·400 | | 615 |
| 127 | 1854·20 | | 56·560 | | 22624 | |
| | | 57·27 | | 1·415 | | 624 |
| 128 | 1911·47 | | 57·975 | | 23248 | |
| | | 58·68 | | 1·430 | | 633 |
| 129 | 1970·15 | | 59·405 | | 23881 | |
| | | 60·13 | | 1·470 | | 652 |
| 130 | 2030·28 | | 60·875 | | 24533 | |
| | | 61·62 | | 1·500 | | 666 |
| 131 | 2091·90 | | 62·375 | | 25199 | |
| | | 63·13 | | 1·520 | | 678 |
| 132 | 2155·03 | | 63·895 | | 25877 | |
| | | 64·66 | | 1·550 | | 694 |
| 133 | 2219·69 | | 65·445 | | 26571 | |
| | | 66·23 | | 1·575 | | 706 |
| 134 | 2285·92 | | 67·020 | | 27277 | |
| | | 67·81 | | 1·600 | | 720 |
| 135 | 2353·73 | | 68·620 | | 27997 | |
| | | 69·43 | | 1·630 | | 735 |
| 136 | 2423·16 | | 70·250 | | 28732 | |
| | | 71·07 | | 1·670 | | 755 |
| 137 | 2494·23 | | 71·920 | | 29487 | |
| | | 72·77 | | 1·685 | | 765 |
| 138 | 2567·00 | | 73·605 | | 30252 | |
| | | 74·44 | | 1·710 | | 778 |
| 139 | 2641·44 | | 75·315 | | 31030 | |

TABLE (*continued*).

| $t$ in Centigrade degrees. | $p$. | $\Delta$. | $g$. | $\Delta$. | $T \cdot g$. | $\Delta$, |
|---|---|---|---|---|---|---|
| 139 | 2641·44 | 76·19 | 75·315 | 1·750 | 31030 | 798 |
| 140 | 2717·63 | 77·94 | 77·065 | 1·770 | 31828 | 810 |
| 141 | 2795·57 | 79·73 | 78·835 | 1·810 | 32638 | 830 |
| 142 | 2875·30 | 81·56 | 80·645 | 1·835 | 33468 | 844 |
| 143 | 2956·86 | 83·40 | 82·480 | 1·865 | 34312 | 860 |
| 144 | 3040·26 | 85·29 | 84·345 | 1·895 | 35172 | 876 |
| 145 | 3125·55 | 87·19 | 86·240 | 1·920 | 36048 | 891 |
| 146 | 3212·74 | 89·13 | 88·160 | 1·960 | 36939 | 911 |
| 147 | 3301·87 | 91·11 | 90·120 | 1·990 | 37850 | 928 |
| 148 | 3392·98 | 93·11 | 92·110 | 2·015 | 38778 | 943 |
| 149 | 3486·09 | 95·14 | 94·125 | 2·045 | 39721 | 959 |
| 150 | 3581·23 | 97·20 | 96·170 | 2·085 | 40680 | 980 |
| 151 | 3678·43 | 99·31 | 98·255 | 2·120 | 41660 | 999 |
| 152 | 3777·74 | 101·44 | 100·375 | 2·140 | 42659 | 1012 |
| 153 | 3879·18 | 103·59 | 102·515 | 2·175 | 43671 | 1032 |
| 154 | 3982·77 | 105·79 | 104·690 | 2·220 | 44703 | 1054 |
| 155 | 4088·56 | 108·03 | 106·910 | 2·250 | 45757 | 1073 |
| 156 | 4196·59 | 110·29 | 109·160 | 2·270 | 46830 | 1085 |
| 157 | 4306·88 | 112·57 | 111·430 | 2·310 | 47915 | 1107 |
| 158 | 4419·45 | 114·91 | 113·740 | 2·345 | 49022 | 1127 |
| 159 | 4534·36 | 117·26 | 116·085 | 2·375 | 50149 | 1144 |
| 160 | 4651·62 | 119·66 | 118·460 | 2·410 | 51293 | 1165 |
| 161 | 4771·28 | 122·08 | 120·870 | 2·445 | 52458 | 1184 |
| 162 | 4893·36 | 124·55 | 123·315 | 2·490 | 53642 | 1209 |
| 163 | 5017·91 | 127·06 | 125·805 | 2·510 | 54851 | 1222 |
| 164 | 5144·97 | 129·57 | 128·315 | 2·545 | 56073 | 1244 |
| 165 | 5274·54 | 132·15 | 130·860 | 2·585 | 57317 | 1265 |
| 166 | 5406·69 | 134·74 | 133·445 | 2·620 | 58582 | 1286 |
| 167 | 5541·43 | 137·39 | 136·065 | 2·670 | 59868 | 1314 |
| 168 | 5678·82 | 140·08 | 138·735 | 2·685 | 61182 | 1326 |
| 169 | 5818·90 | 142·76 | 141·420 | 2·725 | 62508 | 1348 |
| 170 | 5961·66 | 145·53 | 144·145 | 2·765 | 63856 | 1372 |
| 171 | 6107·19 | 148·29 | 146·910 | 2·795 | 65228 | 1390 |
| 172 | 6255·48 | 151·12 | 149·705 | 2·830 | 66618 | 1412 |
| 173 | 6406·60 | 153·95 | 152·535 | 2·880 | 68030 | 1440 |
| 174 | 6560·55 | 156·88 | 155·415 | 2·920 | 69470 | 1464 |
| 175 | 6717·43 | 159·79 | 158·335 | 2·935 | 70934 | 1476 |
| 176 | 6877·22 | 162·75 | 161·270 | 2·980 | 72410 | 1502 |
| 177 | 7039·97 | 165·75 | 164·250 | 3·025 | 73912 | 1529 |
| 178 | 7205·72 | 168·80 | 167·275 | 3·060 | 75441 | 1550 |
| 179 | 7374·52 | 171·87 | 170·335 | 3·090 | 76991 | 1570 |
| 180 | 7546·39 | 174·98 | 173·425 | 3·140 | 78561 | 1599 |
| 181 | 7721·37 | 178·15 | 176·565 | 3·170 | 80160 | 1619 |
| 182 | 7899·52 | 181·32 | 179·735 | 3·205 | 81779 | 1642 |
| 183 | 8080·84 | 184·56 | 182·940 | 3·255 | 83421 | 1670 |
| 184 | 8265·40 | 187·83 | 186·195 | 3·280 | 85091 | 1688 |
| 185 | 8453·23 | 191·12 | 189·425 | 3·320 | 86779 | 1714 |
| 186 | 8644·35 | 194·47 | 192·795 | 3·370 | 88493 | 1743 |
| 187 | 8838·82 | 197·86 | 196·165 | 3·400 | 90236 | 1763 |
| 188 | 9036·68 | 201·27 | 199·565 | 3·445 | 91999 | 1792 |
| 189 | 9237·95 | 204·75 | 203·010 | 3·480 | 93791 | 1814 |
| 190 | 9442·70 |  | 206·490 |  | 95605 |  |

TABLE (*continued*).

| *t* in Centigrade degrees. | *p.* | Δ. | *g.* | Δ. | T . *g.* | Δ. |
|---|---|---|---|---|---|---|
| 190 | 9442·70 | 208·23 | 206·490 | 3·515 | 95605 | 1837 |
| 191 | 9650·93 | 211·78 | 210·005 | 3·550 | 97442 | 1861 |
| 192 | 9862·71 | 215·33 | 213·555 | 3·595 | 99303 | 1889 |
| 193 | 10078·04 | 218·97 | 217·150 | 3·645 | 101192 | 1919 |
| 194 | 10297·01 | 222·62 | 220·795 | 3·675 | 103111 | 1941 |
| 195 | 10519·63 | 226·32 | 224·470 | 3·715 | 105052 | 1966 |
| 196 | 10745·95 | 230·05 | 228·185 | 3·750 | 107018 | 1991 |
| 197 | 10976·00 | 233·82 | 231·935 | 3·795 | 109009 | 2020 |
| 198 | 11209·82 | 237·64 | 235·730 | 3·840 | 111029 | 2048 |
| 199 | 11447·46 | 241·50 | 239·570 | 3·885 | 113077 | 2077 |
| 200 | 11688·96 | | 243·455 | | 115154 | |

LV. *On a new Arrangement of the Induction Coil.*
*By* JONATHAN N. HEARDER, *Plymouth\**.

1. IN my communication to the last Number of the Philosophical Magazine, I confined myself to the announcement of a few of the novel results which I had obtained from investigations with my induction coil. I might have given many more, but purposely refrained from doing so until I had had an opportunity of repeating my experiments with a still more powerful machine, as certain indications had led me to believe that many of the results which have hitherto been announced as peculiar to the action of the induction coil, have been disguised by the mode of construction of the instruments of M. Ruhmkorff; such, for example, as the ignition of the negative terminal only, and the comparative inertness of the terminal proceeding from the inner portion of the coil. These and other effects appeared to me to indicate that M. Ruhmkorff's secondary coil could not be perfectly insulated from the primary one, and that the true statical condition of the two terminals could not consequently be accurately ascertained and compared.

2. It happened in the first arrangement of my machine, that I was led to construct all its elements separate and independent of each other, for the purpose of ascertaining how modifications of any of them influenced the action of the rest. The secondary coil in this case was perfectly insulated, and I was frequently struck with unexpected results, especially in the improved arrangement of my first machine alluded to in my last paper, since both terminals would sometimes become heated, and the inner terminal was possessed of such high statical intensity that it could not be handled with impunity. In a still more powerful machine

which I have recently constructed, and which I am about to describe, these phænomena are more strikingly evident; and but for the arrangement which I have adopted, I believe that they would not have been developed.

3. The machine consists of a gutta-percha bobbin of about 12 inches in length, the cylindrical centre of which is large enough to receive the primary coil with its core.  Upon this bobbin is wound the secondary coil, consisting of fine copper wire covered with silk, and carefully varnished with shell-lac, which is allowed to dry previously to winding on.  The consecutive layers of the wire are separated from each other by a stratum of oiled silk or sheet gutta-percha.  I am thus enabled to wind off and relay the wire to repair accident; and I have more than once found the advantage of the arrangement; for, on forcing my first machine to a very extreme point of tension, the internal insulation broke down, and on winding off the wire I discovered the cause of the accident, and was able to apply the appropriate remedy.  The contact breaker which I employ is similar to that of my medical coil machine, consisting of a very stiff brass spring, armed at one end with a piece of iron, to be attracted by the end of the iron core, and breaking contact by means of a platina stud fixed about the centre of its length, and pressing against a contact screw also armed with platina.  The condenser is, as in Ruhmkorff's machine, enclosed in the base of the instrument. The secondary coil is rather less than three miles in length.  My exciting battery is a modification of Grove's nitric acid battery, the zinc being excited with muriate of ammonia instead of dilute sulphuric acid, by which the amalgamation of the zinc is dispensed with, and the battery is in every respect equally powerful. With twelve cells the following effects are produced with the coil.

4. The spark at the interruptor is loud, brilliant, and flame-like, frequently throwing out a flat sheet of flame of a peculiarly distinctive character, as large as a shilling, or even larger.  By a connecting screw the condenser can be detached, when the ordinary secondary spark appears at the break very different from that which occurs when the condenser is used.

5. Each secondary terminal gives sparks to an uninsulated conductor, which are longer when the opposite terminal is communicating with the ground, sometimes more than an inch in length; and on connecting the terminals with a graduated Lane's discharger furnished with platina points, torrents of sparks in bundles pass between them at a distance of 2·5 inches.  At 2·75 sparks still pass, but not in such quantities; and on modifying the vibrations of the spring with the finger, so as to vary slightly the pressure, occasional sparks will pass nearly 3 inches in length.

6. On approximating the points to within 0·4 or 0·5 of an

inch, both become white-hot, the negative being rather hotter than the positive; and on substituting fine iron wires for the platina points, both enter into combustion. Fine platina wires become white-hot at the ends, melt, and run back into globules.

7. Through the flame of alcohol a flood of sparks will pass between 8 and 9 inches in length, producing a curious zigzag circuit, and accompanied with very loud crepitations.

8. In an exhausted receiver, 3 feet in length and 4 inches in diameter, a splendid ribbon of light is produced of the most exquisite colours, the centre line being bright, and surrounded by an intense crimson light, shading off into violet and purple. I have only yet tried a receiver of these dimensions, but should consider from appearances that the light would pervade a much longer vacuum.

9. I have only tried Gassiot's cascade in a flower vase 10 inches high, but there can be no doubt that a flower vase of 2 or 3 feet in height would be equally well illuminated, as I was obliged to use only 3 or 4 cells, otherwise the action of the machine was too strong. The contrast in the appearance of the light on reversing the current is most beautifully exhibited. On making the inside of the vase positive, the electricity appears to flow in an immense sheet out over the edge to a considerable distance, and fall upon the plate of the pump; but on reversing the current, a totally different appearance is produced: the sheet appears now to envelope the vase more closely, and to flow up and turn in over the rim, the direction of the current in each case being too decided to be mistaken.

10. I have not yet heard of any effects from a single machine which have at all approximated to these results; and as I believe that the arrangement is not only more powerful, but possesses important advantages over that adopted by M. Ruhmkorff, I have not hesitated to bring it before the scientific world. I find it, however, absolutely necessary to identify myself with the process of manufacture, as even the most careful workmen are apt to overlook what may appear to them trifles, but which are, however, in fact the main causes of success or failure. I am also satisfied that the foregoing effects, although of an unusual character, are but trifles compared with what may be obtained from a judicious extension and modification of the principles; and I even doubt whether I have yet attained the maximum effect from the secondary wire of the machine which I have now described, as the secondary current, although increased by extra battery power, depends more upon the magnetic intensity of the iron core than upon any modified conditions of the battery current and primary coil, except so far as they shall combine to exalt the magnetic condition of the included core.

LVI. *Memorandum respecting a new System of Roots of Unity. By* Sir William Rowan Hamilton, *LL.D., M.R.I.A., F.R.A.S., &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*\*.

I HAVE lately been led to the conception of a new system, or rather *family of systems*, of *non-commutative roots of unity*, which are entirely distinct from the $ijk$ of the quaternions, though having some general analogy thereto; and which admit, even more easily than the quaternion symbols do, of *geometrical interpretation*. In the system which seems at present to be the most interesting one, among those included in this new family, I assume three symbols, $\iota$, $\kappa$, $\lambda$, such that

$$\iota^2 = 1, \quad \kappa^3 = 1, \quad \lambda^5 = 1, \atop \lambda = \iota\kappa ; \qquad \Big\} \quad \ldots \ldots \text{ (A)}$$

where $\iota\kappa$ must be *distinguished* from $\kappa\iota$, since otherwise we should have $\lambda^6 = 1$, $\lambda = 1$. As a very simple *specimen* of the symbolical conclusions deduced from these fundamental assumptions, I may mention that if we make

$$\mu = \iota\kappa^2 = \lambda\iota\lambda,$$

we shall have also†

$$\mu^5 = 1, \quad \lambda = \mu\iota\mu ;$$

so that $\mu$ is a new fifth root of unity, connected with the former fifth root $\lambda$ by relations of perfect reciprocity. A long train of such symbolical deductions is found to follow: and every one of the results may be *interpreted*, as having reference to the passage from *face to face* (or from corner to corner) of the *icosahedron* (or of the dodecahedron): on which account, I am at present disposed to give the name of the "Icosian Calculus," to this new system of symbols, and of rules for their operation. Some additional remarks on this subject may soon be offered to the Philosophical Magazine, under the title, already sanctioned by the Editors, of "Extensions of the Quaternions."

Observatory of Trinity College, Dublin,
      October 29, 1856.

---

\* Communicated by the Author.
† In fact, by (A),

$$\iota\kappa = (\iota\kappa)^{-4} = (\kappa^{-1}\iota^{-1})^4 = (\kappa^2\iota)^4,$$
$$1 = \iota \cdot \iota\kappa \cdot \kappa^2 = \iota(\kappa^2\iota)^4\kappa^2 = (\iota\kappa^2)^5 ;$$

also

$$\mu\iota\mu = \mu\kappa^2 = \iota\kappa^4 = \iota\kappa = \lambda.$$

### LVII. *On the Anticlinal Line of the London and Hampshire Basins. By* P. J. MARTIN, *Esq.*

#### *To Richard Taylor, Esq.*

MY DEAR SIR,

A S you have with much kindness hitherto afforded me space in the Philosophical Magazine for the publication, *in extenso,* of everything I have had leisure to bring before the public on the much-vexed question of the Weald Denudation, I beg the favour of a continuance of your good offices in the same direction.

By some the subject may be considered rather stale; but so long as there exists a doubt on the public mind of whether we are to refer the phænomena of the great denudations around us to the slow sempiternal erosion of weather and water, as we see them now in operation, or to call in the assistance of seasons of paroxysmal violence and activity under other conditions, any contribution which tends to throw light on such a subject cannot be viewed with indifference.

<div align="center">I am, dear Sir,<br>Your obliged friend and Servant,<br>P. J. MARTIN.</div>

---

In my description of some of the parallel lines of elevation which constitute the great anticlinal, and by their contortion broke up the superficial strata and prepared them for removal, at p. 50. vol. ii. 4th series, Phil. Mag., will be found the following foot-note:—" Of the manner in which these parallel lines may pass uninterruptedly and sometimes silently through a country, we have a good example in the case of Portsdown Hill in Hampshire, High Down near Worthing in Sussex, and the cliffs of Seaford in the same county, all elevations of the same character, lying in the same course east and west, with a dip opposed to the prevailing one of the intervening country. An elevatory force acting with greater intensity at these points can alone explain the coincidence." I had previously described these eminences in my publication in 1828 as chalk *outliers-by-protrusion.* Within the last two years a better acquaintance with the country has shown me that the line in question does not pass through it so clandestinely, but that the chalk is brought to the surface, obscured here and there by " drift " in all the line of country south of the South Downs from Seaford by High Down, through the Manwode, south of Chichester, and by Portsdown into Hampshire, as far as Fareham and Titchfield, where it is lost under the Hampshire tertiaries. In all this course of at least sixty miles, tertiary formations are to be found

north and south of it. As I have elsewhere said, it is variable in its character. At the points indicated, as above, by the remarkable eminences of Portsdown and High Down, the dip into the northern synclinal is gentle and easy; but it is in others more abrupt and inclining to the character of what Mr. Hopkins calls flexure*, like the one-sided saddle at Farnham and the Hogsback in Surrey, or even to direct upcast or fault; for in its passage from Portsdown eastward by Emsworth, I find that on the south side of the hamlet of Prinsted, wells reach the chalk through ten or twelve feet of gravel; whilst on the north side of the same, within the short distance of 200 yards, wells are sunk 60 or 70 feet in London clay, and then water is only procured by percolation from above. I have not at present evidence to prove that this line of elevation has so much the character of a "fault" or sharp flexure elsewhere, but there is good reason to believe that this is very much the case in all the line of country through which it can be traced between Portsdown and High Down. However this may be, it is certain that all the flat country south of the Downs, ranging from Bedhampton at the foot of Portsdown to Brighton, including Hayling and Thorney Islands, the estuary of Chichester Harbour, and the peninsula of Selsey, is intersected by this chalk ridge with a trough of tertiaries on each side. The importance of this arrangement cannot be too highly appreciated, as bringing so great an accession to that category of parallel linings or foldings of which the great anticlinal is composed, and carrying the phænomena of denudation along with it so far out of the range of the Weald Valley. This great tract of country has been so completely *rasée*, it is so flat and so copiously bestrewed with drift, and so little raised above the level of the sea, as to have impressed some early observers with the notion of a sea-bed comparatively modern, of which the chalk downs were the cliff-bound coast. It is unnecessary to say that this idea is here entirely repudiated. The enormous quantity of angular flint, the entire removal of the chalk to which that flint belonged, the thick beds of fertile loams and brick-earths manufactured on the spot and mixed up with these angular flints, the absence of rounded gravel except such as has been obviously derived from the eocene immediately contiguous to the chalk,—all bespeak a tumultuary activity by far transcending the erosion and attrition of an ordinary sea-board.

The drift of this flat and remarkable district admits of a natural division into two kinds—the diluvial, and the boulder or glacial. The former is the loamy drift with angular flints, as before said, so copiously spread over the northern division. The southern, comprising the peninsula of Selsey, and all the coast of Brackles-

* Transactions of the Geological Society, vol. vii. p. 17.

ham Bay to Chichester Harbour, is occupied by the boulder-drift. The line of demarcation of these two kinds of drift runs nearly parallel with, and close upon the protruded chalk-anticlinal above described\*. I have not been able to find evidence of the existence of the boulder-drift west of Chichester Harbour, in Thorney or Hayling, and it dies out as we come north from Selsey, though there is ample proof of its ancient extension over a much larger surface; blocks of granite and of other crystalline rocks are found at Siddlesham, Hounston Common, and in Birdham. The occurrence of these granitic boulders on the Sussex coast was first noticed by Mr. Dixon in his summary of the Geology of the Bracklesham district†; he does not seem, however, to have been aware of their importance, or to have understood the true geological relations of the deposit in which they were found. Mr. Godwin Austen has also recognized the existence of this deposit, and has described and speculated on it in a paper read before the Geological Society, of which an abstract is given in the 'Quarterly Journal,' vol. xii. p. 4. It is composed of loam or brick-earth, extensive beds of rounded gravel and angular flint, the former much predominating, with here and there large erratic blocks, the whole exhibiting the heterogeneous and tumultuary character of the boulder-drifts of the eastern counties. Of the position and the true geological relations of this important deposit more will be said by-and-bye; for the present it will be well to return to a further consideration of the angular flint, or—

*Diluvial Drift.*—Much valuable detail of the nature and distribution of this drift has been given by Sir R. I. Murchison in his paper "On the Flint Drift of the South Downs" (Quarterly Journal of the Geological Society, Feb. 1, 1852). The soil of the greater part of the flat country under review is entirely composed of it, and it lies especially deep between the base of the Downs and the chalk-anticlinal above described. An attentive study of the country, with a due consideration of the nature of the stratified beds below, and of the convulsive changes they have undergone, must bring the conviction that the loams, brick-earths, beds of impacted angular gravel, and all the other superficial phænomena of this deposit, are the result of diluvial currents operating through geological periods more or less prolonged, and recurring at longer or shorter intervals.

Good illustrative sections of these gravels are to be seen on each side of the road from Slindon Common, where Sir R. Murchison says they appear in great force, to Chichester. At Ball Hut they are loosely mixed up with the sands of the lowest

---

\* This line of demarcation is not sharp and well defined, but there is a manifest and marked difference north and south of it.

† Dixon's 'Geology of Sussex,' p. 14.

tertiary, a pit of which was some few years since open by the road side near the nursery garden. Further west, at Crocker Hill and Boxgrove, the angular flints are largely mixed up with eocene shingle, and so it continues on to Goodwood. At West Hampnet they are closely impacted with clays derived from the stratum above mentioned. Further on, between the workhouse and the city, close by the Lavant, and near the public-house at the turning to Goodwood, a pit was open three or four years ago, from whence flints were turned out with so much chalk-rubble about them and so white as to look like chalk-stones. I have not seen anything like it except at Brighton, and under the Downs at Boxhill in Surrey. Further west in the Portfield the gravels are again smaller and more washed. At Rumboldswick, again, they become larger, and the same in the direction of Donnington and Appledram, and are here again mixed and impacted with the subjacent clays. South from these points the surface of the Manwode country is chiefly tertiary clay and sand, till the boulder-drift falls in from the coast.

If any stress be laid on the difference of colour in these gravels as indicative of their age or of their æra of formation, let it be admitted. The flints by the side of the Lavant are not so much pounded as those at the West Hampnet brick-yard, where they are for the most part broken into small fragments and impacted with clay-loam; and they are white, because they have not been washed out of the chalk rubble that surrounds them. Even their superposition, if it be clearly made out, argues nothing more than that they have been outspread after the washing, smashing, and impaction of the brown (brown because mixed up with ferruginous) clay, near or on which they lie. Unwashed flints are spread out in like manner at Brighton, in the valley of the Mole near Boxhill, and on the outskirts of the foot of the North Downs from Dorking to Reigate;—and are here and there to be found in chalk-rubble drift on the plateau of the upper greensand in West Sussex. This white gravel, if it may be so called, was brought into notice in my former publication on tertiary drift (see Phil. Mag. for October 1851); and its superposition, be that granted, argues nothing more than another diluvial sweep,—else, why is it, and how came it there?

West of Chichester, towards Emsworth and the foot of Portsdown Hill, the same features are observed, more brick-earth and clay-impacted gravels, the stratified London or Plastic clays appearing here and there on the surface. From this part of the country, along the Forest of Bere, and skirting the Downs, this band of angular flint-gravel, sometimes loosely and sometimes closely bound together, and mixing with eocene shingle, crosses over the chalk-dome of the Alton Hills, and uniting here and there

with the *débris* of the tertiaries still persistent on the chalk, to the back of the North Downs, where the same zone is resumed with the same appearances, and extends from Guildford to Canterbury. There is an important break in the series between Basingstoke and Guildford, owing to the peculiar upcast of the chalk between Crondall and the last-mentioned place. With this exception, the same general character is to be observed in all this spread of supracretaceous gravel. I have dwelt more on these phænomena, because they are, and especially in the Chichester country, so strikingly opposed to every notion of gentle attrition and gradual abrasion.

Neither at Chichester, nor in any part of the great plain in which it stands, are there any terraces or signs of modern sea-beds properly so called. And with regard to the sudden rise of the chalk downs out of this level and deceptive surface, it is to be attributed to two causes. The first, the natural prominence of a stoney stratum in a strongly denuded country. The second, the vicinity of some subordinate anticlinals, or other causes of intumescence running from Bowhill* by West Dean, Singleton, and East Dean, some signs of which are to be seen in a chalk quarry at the top of Duncton Hill†. East of this country, and on towards the Adur, the hills are tilted, as I have elsewhere explained, by the long anticlinal of Greenhurst which rules the escarpment of the Downs from Duncton to Wolfstanbury. It is this sudden rise of the chalk out of the plain which Sir C. Lyell has figured in his 'Manual' as illustrative of a line of sea-cliffs (p. 276, 5th edit.),—a position I conceive to be quite untenable. The chalk was hard, the tertiaries soft and destructible; a strong denuding force brought the former forward with more than usual prominence, the tilting of a proximate line of elevation assisting in the work. With all but religious faith in the doctrines of the author of what may be called the philosophy of modern geology, I grieve to find that our theories of the Weald can be made to approximate in so few points. Almost all the illustrations Sir C. Lyell has brought to bear on his own views, I consider, like this, to be fallacious, and founded on an erroneous principle.

The best approach to a true theory of the denudation has been made by my friend Dr. Fitton, in his little book on Hastings. But he has done his work too timidly.

---

* See Ordnance Map.

† Another curious illustration of this line of disturbance and excavation exists in a miniature mole which runs along the valley. The infant Lavant is here a mere winter-bourne, and its channel fills only after long-continued rains. I am credibly informed that it makes its appearance at Singleton, and runs strong there, and disappears in an adjoining meadow, breaking out again about a mile off at West Dean, for a fortnight before it fills its channel full the whole way.

Mr. Hopkins walls himself up, as I have elsewhere said, in an imaginary line of demarcation, and fails of carrying his speculations out to their legitimate conclusion. He breaks up the Weald, but does not dispose of the materials. Does he leave them to be disposed of by the gentle means of erosion, by seaboards or under-currents, or by atmospheric wear? The erosion by sea-boards it cannot be, for there are no beaches; there are lacerated escarpments, but there are no cliffs; there are chasms and ravines, but these do not bear the marks of having been "fiords" or estuaries; and there are bare rocks, but these have no stratified beds with their attendant fossils at their feet, nor are accompanied by any of the other ordinary signs of sea-beds.

<div style="text-align:center">[To be continued.]</div>

LVIII. *On the Importance of an Adequate Definition in developing the Theory of Parallel Lines.* By J. P. HENNESSY*

PROFESSOR STEVELLY'S criticism, in the last Number of the Philosophical Magazine, on a paper of mine which was published nearly four years ago, raises two distinct questions. It raises, in the first place, a question as to the logical value of the reasoning in my paper. On this I do not think it necessary to offer any observations. To do so, would be to approach too close to what every man of science must wish to avoid,—a personal discussion. I am quite certain that a discussion of such a nature could have no claims whatever to occupy the valuable space of the Philosophical Magazine. I trust, therefore, that Mr. Stevelly will not feel I am treating him with any discourtesy if I allow all the assertions as to "defective logic and incorrect reasoning" to pass by unanswered†.

But the position taken by Mr. Stevelly raises another and more important question. It brings forward the very point on which, in my opinion, the Theory of Parallels will be found to depend. This is, the question as to the sort of definition we are at liberty to employ.

---

* Communicated by the Author.

† With regard to the opinion I may happen to entertain as to the details of his demonstration, Mr. Stevelly observes,—"I am much gratified that Mr. Hennessy does not profess to have detected any defect in the chain of reasoning on which the proofs are founded." For fear of misapprehension, I may remark that I did not offer any opinion on that subject, for a very simple reason. When I had pointed out that the whole principle of Mr. Stevelly's method was taken—inadvertently I am sure—from a French geometer, it did not appear necessary to go any further. The minor details of a "New Method," which was thus deprived of all claim to originality, could hardly be expected to command much attention.

Mr. Stevelly's position is by no means a novel one. In looking over the voluminous history of the subject, one is struck by the large number of persons who object to anything like a full and adequate definition. The objection is generally made on the ground that the definition "assumes" something; or that it contains a "property;" or that it "involves a *petitio principii*." It is hardly necessary to say, that such objections imply a misapprehension of the first principles of formal logic. In framing a definition, something must be assumed; and that something must be a property. When the property is incorporated in the definition, it becomes its differentia. In speaking, as logicians, of the properties of any conception that we may have thus defined, the differentia is no longer included. The properties are all susceptible of being deduced from the definition; but, to expect that its own differentia should be deduced from it in a similar manner, is, undoubtedly, a grave mistake. Such a mistake will arise from a confusion between the differentia and the propria; and it appears to have, not unfrequently, impeded the proper settlement of this question. For example, Mr. Stevelly says, referring to the definition in which a case of the 29th proposition is made the differentia (that the two interior angles on the same side will be together equal to two right angles),—" The 29th proposition of Euclid's Elements calls on me to prove that 'any' line, which cuts those [two parallel lines], also makes the two interior angles on the same side together equal to two right angles. This is a relation of these two lines to all others which cut them which requires to be proved, and which neither the above definition nor any other definition can warrant us to assume as a truth without proof. It is a 'property'* which must be shown to be an essential concomitant of that which has already fixed the relation of those two lines to one another, and to all others which cut them, and which we have by the definition merely agreed shall entitle us to denominate them 'parallel lines' in any individual case in which we find it to obtain." Here, in consequence of a confusion of the differentia and propria, the remarkable oversight is committed of expecting a proof of the very thing that has been assumed, and on which any demonstration must rest. It would be quite impossible to give a demonstration of the differentia and propria of any complete geometrical conception. The proprium chosen for a differentia becomes an ultimate premiss in the sorites. As far as susceptibility of proof is concerned, it is altogether outside the domain of demonstration.

* Had Mr. Stevelly called it by its right name, the " differentia," he would at once have seen his mistake.

In attempting to decide on the differentia of parallel lines, or of any other geometrical conception, it is of importance that we should carefully ascertain how far we are privileged to go; that is, we should ascertain how much a mathematical definition is permitted, and expected, to include. It is obvious that the nature of the definition will depend in a great measure on whether we believe, on the one hand, that "it entitles us merely to denominate certain lines parallel in any individual case;" or whether, on the other hand, there is something besides the mere meaning of a term, as a nominal definition, involved in it. The views of Dr. Whewell on this subject are well known. He* has always upheld the real character of mathematical definition. Even Mr. Mill recognizes† something more than an arbitrary name. There is, however, no necessity to go further than the writings of Dr. Whately to find the strongest confirmation of the views I have ventured to put forward. "The term definition," he says, "is used with some laxity; and much confusion has thence resulted. Such definitions as the mathematical, *must imply every attribute* that belongs to the thing defined." Again, he says,— "In mathematics there is no distinction between nominal and real definition; the meaning of the term and the nature of the thing being one and the same : so that *no correct definition whatsoever of any mathematical term can be devised, which shall not imply everything which belongs to that term."*

The next point to determine is, whether we are at liberty to take any adequate proprium we choose for the differentia. That we are entitled to do so there cannot be the slightest doubt. On the plainest logical principles, it would be absurd to suppose that any property, possessing sufficient comprehension and clearness, could be, by any arbitrary arrangement whatever, distinguished from the others, and set aside as incapable of being employed in definition. And, on the other hand, we are fully at liberty, in case any advantage appears to be gained by such a course, to strip a definition of its discretive form, and to place it amongst the propositions‡.

It would be indeed very difficult to point out any striking proprium of parallels which has not at some time or another been made a differentia, by the second class of geometers, alluded to

---

* Philosophy of the Inductive Sciences, vol. i. p. 92; vol. ii. pp. 598,599.
† In speaking of the definition of a circle (System of Logic, vol. i. p. 336).
‡ The very course here alluded to has been taken by many Continental geometers. In this country, amongst others, the author of the article "Geometry" in the *Encyclopædia Metropolitana* has adopted it. After giving a definition, practically the same as Wolfius's, he observes,—"We have preferred the definition above given, and have made the property of parallel lines never meeting a proposition instead of a definition."—*Pure Sciences,* vol. i. p. 313.

on a former occasion*. M. Varignon, M. Etienne Bezout and many
others, have suggested that case of the 29th proposition of Euclid,
which states that the exterior angle is equal to the interior and op-
posite. From this, down to Mr. Stevelly's, we have had definitions
of all sorts.    The list commences with such as fulfil Dr. Whate-
ly's idea of what a mathematical definition should be, and ter-
minates with such as are inadequate and useless.    There is one
test that every logician would employ in examining the practical
value of ordinary definitions, which should be applied to these.
It depends on the relative ease with which we find the properties
flow from the definitions.    The number of writers who have
gone out of their way to get a definition which involves in the
deductions from it, to use Dr. Lardner's phrase, " a labyrinth of
complicated and indirect demonstration," is remarkable.

For the purpose of assisting in the choice of a definition of
parallels, I made an analysis of the definitions of a circle and a
square. The advantages derived from an examination of Euclid's
definitions are not confined to the development of the analogies
then found to exist.    Let us, for instance, take four of the most
important ;—

I. *Right line* :—That which lies evenly between its extremities.
II. *Right angles* :—When a right line standing on another right
        line makes the two adjacent angles equal.
III. *Circle* :—A continued line, having a point within it, from
        which point all right lines to the continued line are
        equal.
IV. *Parallel lines* :—Such as cannot meet when produced.

Of the first it is sufficient to say that it is not framed in the
accurate language of geometry.    In fact a geometer, as such,
can attach no meaning to the phrase lying " evenly."    The de-
finition has therefore never been used.    The second and third
are clear, adequate, and perfectly logical.    The fourth is certainly
clear enough, but it appears to be inadequate, and, as a defini-
tion, to be illogical.    To remedy the defect of the first and
fourth, Euclid has given two axioms.    The necessity for employ-
ing some positive property in treating parallel lines is thus clearly
stated in Dr. Whewell's ' Essay on Mathematical Reasoning :'—
" We must have some positive property, besides this negative
one, in order to complete our reasonings respecting such lines.
We have, in fact, our choice of several such self-evident proper-
ties, any of which we may employ for our purpose, as geometers
well know ; but with our naked definition, as they also know,
we cannot proceed to the end."    Instead, however, of choosing
some positive property of parallel lines, Euclid, unfortunately,
attempted to meet the difficulty (a difficulty arising in the first

* Phil. Mag. vol. xii. p. 372.

instance solely from inadequate definition) by an indirect course.
All straight lines drawn in the same plane are lines which will
(1) either meet (2) or not meet; and, instead of stating some
positive property of the second class, Euclid gives a property of
the first class.   His definition, for such we may call it, of lines
that will meet, may have involved, as its *differentia*, other pro-
perties instead of that which he has chosen.   There can how-
ever be little doubt that for the immediate purpose in view—a
definition of lines that will meet—no property could be better
chosen than the one which appears as the differentia of the so-
called 12th axiom.

With these remarks we may proceed to consider the immediate
difficulty with which geometers have to deal.   It appears to be this:
Euclid's 35th definition is acknowledged to be defective, the 12th
axiom has also been condemned, and the question arises, with
what *single definition* can we replace both of these?   Upon the
answer to this question must depend our method of treating the
theory of parallels.   Enough has been stated in this, and in
former communications, to indicate the course I would take the
liberty of suggesting.   It is, that a full and adequate definition
should be laid down; such a definition, for instance, as that given
by Varignon, in which a case of the 29th proposition is proposed
as the differentia.   In substituting a definition for the 12th
axiom, the example of Euclid may, however, to a certain extent,
be followed with advantage.   I venture to think that we should
deviate from the course adopted by him, to the extent of stating
a positive property of parallel lines instead of stating a property
of lines that are not parallel; but, in other respects, the defini-
tions may be, as nearly as possible, similar.   If so, the defini-
tions, compared together, would stand thus:—

*Lines which meet,*— make, with an intersecting line, two interior
    angles together less than two right angles.

    For which we are to substitute,

*Parallel lines,*—make, with an intersecting line, two interior
    angles together equal to two right angles.

This definition of parallels has no practical advantage over
Varignon's; but, as it is probably the one which Euclid himself
would have chosen had he to remove the 12th axiom, we ought
perhaps to retain it, on the ground that any alteration of his
' Elements' should be as slight as possible.

This definition is not arbitrary; it is not merely an assumed
form of words.   It is *necessarily* involved in a fundamental con-
dition under which the mind contemplates the first truths of
geometry, a condition prior to experience, and independent of it,
—the idea of space.   It may therefore, perhaps, be said that it
implies an axiom as well as a definition.   Such an objection, I

apprehend, would be only verbal, for it amounts to saying that it contains a real as well as a nominal definition. It is an objection which may be raised in regard to any adequate mathematical definition whatever. " We might," says Dr. Whewell, " begin by defining a right angle to be the angle made by a line which stands evenly between the two portions of another line ; and we might add an axiom, that all right angles are equal. Instead of this, we define a right angle to be that which a line makes with another when the two angles on the two sides of it are equal. But in all these cases we express our conceptions of a necessary relation of lines ; and whether this be done in the form of definitions or axioms, is a matter of no importance."

That Euclid's 12th axiom is the same in kind as his definition of a circle, right angles, &c., has been universally acknowledged. In the celebrated controversy on the grounds of mathematical reasoning, D'Alembert calls it a definition, and, except in the mere question of nomenclature, which cannot affect the point I have attempted to establish, his opponents agree with him. It appears to be of some importance that we should bear this fact in mind when discussing the theory of parallels.

The Cloisters, Inner Temple,
November 8, 1856.

---

LIX. *On the Oxidation of the Constituents of Ammonia by Porous Media, with some Remarks on Nitrification.* By Prof. Schön-bein *of Basle\**.

MY DEAR FARADAY,

.... SINCE I last wrote to you I have actively continued my researches on the phænomena of contact, and obtained some results which are curious enough. You know perhaps, that, according to my former experiments, ozonized oxygen at the common temperature oxidizes both the elements of ammonia, forming with that compound nitrate of ammonia ; whilst, as you are well aware, common oxygen under the same circumstances does not at all affect either gaseous or aqueous ammonia. The same oxygen, however, on being put in contact with certain matters, acquires the power of engendering, with ammonia, nitrous acid, *i. e.* nitrite of ammonia. Platinum and copper are such matters. Moisten the former metal (in the state called platinum black) with a strong solution of ammonia, leave these substances exposed for a short time to the action either of common oxygen or atmospheric air, then treat the metallic powder with some distilled water, and you will easily detect in the liquid the pre-

* Communicated by Professor Faraday.

sence of nitrite of ammonia. The simplest way of doing so is to add to the water some dilute sulphuric acid, and paste of starch containing a little pure iodide of potassium (free even from the slightest trace of iodate); nitrite being present, the mixture will become dark blue.

Assisted a little by heat, even compact platinum is capable of causing common oxygen to produce a nitrite with ammonia. Put some drops of a strong solution of ammonia into a bottle containing air, introduce into the vessel the heated coil of a thick platinum wire, hold over this coil a strip of filtering-paper to which sticks starch paste containing some iodide of potassium, and on being acidulated with dilute sulphuric acid, you will perceive the paste instantaneously turn dark blue. Whilst the hot platinum coil remains within the bottle, whitish vapours make their appearance, which, on being taken up by distilled water, give to that fluid all the properties of a nitrite solution. On acidulation with dilute sulphuric acid, it deeply and instantaneously blues the starch paste containing the iodide, and a strong reaction will be obtained, though the heated coil may have remained in the bottle but for a few seconds. The platinum coil does not require to be heated to redness to produce this effect, but those of iron wire, &c. must have that temperature to enable oxygen to produce a nitrite with ammonia.

I mentioned above copper as another substance which was capable of causing ordinary oxygen to oxidize both the elements of ammonia at the common temperature, and I may add that its action even surpassed that of platinum. To convince yourself of the truth of my statement, put about 50 grms. of minutely divided copper (such as is obtained by reducing oxide of copper by hydrogen) into a bottle containing oxygen or air, moisten the metallic powder with a solution of ammonia, close or cover the bottle, and you will soon see the vessel fill with whitish fumes, which are nitrite of ammonia; for if you introduce into the bottle a strip of paper covered with acidulated starch paste containing some iodide of potassium, it will rapidly be coloured blue. Or if strips of filtering-paper impregnated with distilled water be suspended in it but for a short time, they will contain perceptible quantities of nitrite of ammonia, as you may easily satisfy yourself by applying the above-mentioned test. Even a moistened glass plate or watch-glass used to cover the vessel will receive within a very few minutes so much of the nitrite formed as to enable you to ascertain its presence by the most striking reactions. To complete my statement, I must not omit to mention that copper powder, soon after having been moistened with liquid ammonia, exhibits a rise of temperature, no doubt resulting from the formation of the nitrite of ammonia.

The blue liquid obtained on shaking copper powder with aqueous ammonia and oxygen or atmospheric air, also contains, besides oxide of copper, nitrite of ammonia; for if you add some soda to it and boil it to drive off the ammonia and throw down the black oxide of copper, a solution is obtained which, after being evaporated to dryness, leaves behind a yellowish salt which consists principally of nitrite of soda. This substance, on being mixed with powdered charcoal and heated, deflagrates, yields with sulphuric acid strong fumes of nitrous acid, rapidly discharges the colour of indigo solution strongly acidulated by oil of vitriol, and colours brownish a solution of protosulphate of iron containing free sulphuric acid, &c. Common, pure, or atmospheric oxygen, on being put in contact with copper powder and aqueous ammonia, is so rapidly absorbed that I succeeded in depriving completely a whole cubic foot of atmospheric air of its oxygen within a few minutes. Copper and ammonia may therefore be used as eudiometric agents, and for the preparation of nitrogen from common air. The facts above stated appear to me to bear closely upon the important question of nitrification, proving, beyond any doubt, that, under the influence of the contact of some ponderable matters, inactive oxygen is capable, even at the common temperature, of oxidizing both the constituents of ammonia. Before long I hope to be able to give you some more details on nitrification, a chemical phænomenon which at this present moment deeply engages my attention. . . . .     Yours most faithfully,

Basle, Nov. 10, 1856.                     C. F. Schönbein.

LX. *Some Remarks on a System of Geometry derived from an Analysis of our ideas of Direction and Position. By* H. Wedgwood, *Esq.**

THE cardinal definitions on which I find that the whole fabric of demonstration may be made to rest are substantially these:—

A straight line is a line continuing throughout in the same direction.

Parallel straight lines are lines lying in the same direction from given points in the course of each respectively, though not forming part of the same straight line.

A plane is a surface passing through every point which can be reached from a given point by motion transverse to a certain constant direction, that is, through every point which can be reached from a given point without motion in a certain constant direction.

It will be seen that the whole of these definitions depend upon

* Communicated by the Author.

the conception of sameness of direction at different points of a system; the species defined in the two first being traced out by a point actually moving in the same direction under certain conditions, and in the third by a point moving under the sole condition of a negation of motion in a certain constant direction.

When a point is made to describe a line of any shape, it must begin to move in some one direction; and as long as the stroke is continued in the same unaltered direction, the result will be a straight line under the proposed definition. So if a straight line AB be given, and it be required to draw a straight line parallel to it through a point C, the definition instructs us to draw the line CD in the same direction from C with that marked by the straight line AB at A. If the problem be to draw a plane through A under the guidance of the definition, a direction AN must be given in which the generating point is to be wholly void of motion on setting out from A; and if B be any subsequent point in the plane, the generating point in passing through B must be wholly void of motion in a direction identical with that marked by the line AN at A.

As the motion of a point along a straight line is constantly directed to the point at the extremity of the line, little difficulty is commonly felt with respect to the first definition, but an ambiguity is suspected when the term is applied to parallel lines. It is asserted that the only meaning of the sameness of direction of two straight lines passing through the points A and B, and having no points in common, is the equality of the angles made with the straight line joining AB; and it is asserted, that there is a covert assumption that if the straight lines make equal angles with AB, they will make equal angles with a straight line intersecting the parallels at any other points.

To others, perhaps, the objection does not present itself in so definite a form; but they feel uncertain whether the expression of 'sameness of direction' be not used in a somewhat different sense in respect of parallel lines than when applied to successive portions of the same straight line, and in the former case they are inclined to doubt whether the standard of identity of direction be not derived from the notion of parallelism rather than *vice versâ*.

In answer to these objections, I propose to indicate the sense in which identity of direction is uniformly to be understood in these definitions, and the reasoning founded upon them, by reference to a moveable scale which admits of application to directions measured from different points in a system, and enables us to form an estimate of their identity or divergence.

One main cause of the difficulty which has been felt in determining the elementary composition of the simplest species of

geometrical figure is, I believe, the fact that the knowledge of position and figure is acquired by the exercise of distinct faculties—sight and touch; and thus, while we are seeking the ultimate analysis of the conceptions in question, our attention is distracted by the two classes of phænomena. Now it is well known that the born-blind are as capable of geometrical reasoning as those in full possession of their faculties. It is plain, then, that all that is necessary for the groundwork of demonstration must be given independent of sight; and it will tend to simplify the inquiry into the real foundations of the science, if we place ourselves in the mental condition of the blind, and consider all knowledge of position for which the student must have credit in the course of demonstration, to have been acquired by actual motion through the several points of the system to which the reasoning relates.

The positions ascertained by such a course of experience will of course have reference, in the first instance, to the station of the observer at the commencement of the experiment; and in order to provide him with a definite point of departure, we shall suppose him to make some one point in his own body coincide with a certain point A in the external system. The structure of his bodily frame will then supply him with three standard directions transverse to each other, up and down, right and left, fore and aft, diverging from the point A in the external system, and by reference to these he will distinguish any intermediate direction. Now let the observer begin to move in any direction, in the direction due forwards for example, and by such a motion let him arrive at a second point B. He will carry with him the consciousness of the same standard directions, up and down, right and left, fore and aft, which will now be recognized as diverging from the point B instead of A. But as these directions have reference only to the structure of the observer's own body, it is necessary, in order to preserve any correspondence between the analogous directions in the external system at B and at A (between right and left at B, and right and left at A, for instance), that the observer should avoid all rotation of his own body in the external system during the passage from A to B. To that effect we shall introduce the condition, that while he is moving from one point to another in the external system, no change shall be made in the direction of the point towards which he is moving, as measured on the scale of his bodily structure; that is to say, that if B be directly in front of the observer when he begins to move from A, his motion shall be such as to keep that point directly in front as long as he is moving towards it. With this condition as to the mode in which the observer is supposed to make himself acquainted with the system submitted to his

investigation, the directions diverging from any point in the system must be understood as defined by reference to the standard directions of the observer's own body; and a direction in the external system at B will be the same with a given direction at A when it occupies the same position in respect of up and down, right and left, fore and aft. A straight line in the external system extending directly in front of the observer, or directly to his right when he is stationed at B, will be recognized as lying in the same direction with a straight line directly in front or directly to the right when stationed at A; and so of straight lines extending from A and B respectively, and lying in similar intermediate positions between fore and aft and right and left.

Now let us examine the import of these conditions applied to the case of straight lines AC, BD passing in parallel directions through the points A and B respectively. The meaning of parallelism under the definition will be, that the line BD occupies the same position among the standard directions in the body of an observer moving from A to B without rotation in the system, with that which was filled by the line AC when the observer was at A. On thus comparing the directions of AC and BD by the standard of his own bodily structure, the observer must pass from A to B by some definite path, to which alone the relation of parallelism would seem to refer. But what if he had gone by any other track? Would he then have found the line BD in the same position among the lines of his own bodily standard?

Here we have the condition that the observer is brought to the same point B by a straight line AB, and by a track of some other nature; and as points are known in geometry by position alone, it must be supposed that the position attained by the straight line AB is recognized as identical with that attained by the other track. Let AB be supposed, for example, to extend directly to the right of the observer in his original position at A, whence it follows that A will lie at the distance BA directly to his left when he arrives at B. Then if X be the point to which he is brought by the other track, it is plain that he cannot identify the points B and X without being sensible, when situated at X, that A is placed directly to his left at a distance equal to BA. Thus it appears that an observer moving, under the necessary conditions, from the same original position in a system at A, will be similarly placed in the system at any second point B, whether he attain that point by a straight line or by a track of any other description; and any fixed straight line in the system passing through B will in either case occupy the same position among the directions of his bodily scale. Thus the straight lines AC and BD may be considered as lying in the same directions at A and B respectively, independent of any reference to

the particular path by which the observer may pass from one of those points to the other. But if the directions of the lines at A and B can be conceived as absolutely identical, the lines will also be in the same direction with each other at any other points C and D, inasmuch as each straight line is everywhere in the same direction.

LXI. *Reply to a Note of* Mr. Joule, *contained in the November Number of the Philosophical Magazine.* By Prof. Clausius\*.

IN the November Number of this Journal a note appears from Mr. Joule, which refers to a passage in my memoir "On the Application of the Mechanical Theory of Heat to the Steam-engine," where the experiments conducted by Mr. Joule and Mr. Thomson are mentioned. The note concludes with the following words :—"The results we have arrived at in our joint investigation have tended certainly to develope our views, and to give a more definite knowledge of the constitution of elastic fluids, but they do not contradict our original statements, published before the appearance of Prof. Clausius's papers." I can only explain this note, and particularly the conclusion of it, so far as Mr. Joule himself is concerned, by referring it to a misunderstanding of my meaning. But as I believe that I myself, by an inaccuracy of expression, have given rise to this misunderstanding, I feel it to be my duty to say a few words here in explanation. I believe that the note has been chiefly called forth by the following words in my memoir, which precede the reference to the experiments mentioned :—" Even the physicists which had occupied themselves more especially with the mechanical theory of heat did not at that time (1850) coincide with this view of the deportment of vapour." As Mr. Joule, so far as I know, has nowhere published his views on the deportment of vapour, these words could not of course refer to him ; but I ought perhaps, in order to avoid such a misunderstanding, have expressly limited the statement to such physicists as had applied the mechanical theory of heat to vapours at their maximum density.

With respect, further, to the deportment of gases, which, in the portion of my memoir already referred to, is brought into connexion with the deportment of vapours, I never entertained the least doubt that Mr. Joule, in his beautiful investigations "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," had started from a perfectly correct point of view, and that the results of the new experiments above mentioned do not contradict his former statements, but, on the contrary, have served to confirm them in all essential particulars.

Zurich, Nov. 16, 1856.

\* Communicated by the Author.

## LXII. *Notices respecting New Books.*

*Matériaux de Construction de l'Exposition Universelle de* 1855. *Par*
A. Delesse. Paris, 1856. Pages 420.

THIS is a valuable report on the building materials of all kinds,
whether natural or artificial, which were displayed at the Paris
Exhibition. M. Delesse was peculiarly qualified to undertake the
task of drawing up this report, from his intimate acquaintance both
with geology and chemistry, and from his former researches on the
chemical composition of rocks. The report contains the results of a
large number of chemical analyses, as well as of experiments on the
strength and other physical properties of the materials exhibited.
We may refer particularly to the chapters on marbles and cements,
as containing a large amount of novel and interesting information.
From the latter we extract the following observations on the cements
of Canada.

*Thorold Cement.*—This cement, from Thorold near St. Catherine
in Upper Canada, is prepared by calcining an argillaceous limestone
of the Upper Silurian formation. The limestone is grayish-black,
and emits an argillaceous odour when breathed on; but the cement
itself has a yellow colour. The specimen examined did not effer-
vesce with acids, and contained only 3·37 per cent. of water. It
set in fifteen minutes with a disengagement of heat. A specimen
immersed in water ten minutes after being moistened, set under the
water, and presented finally the same cohesion as another specimen
which was not immersed till two hours after it had set. The resist-
ance to traction of this cement was about 0·85 kil. It was found to
consist of—

| | | |
|---|---|---|
| Lime | 53·55 | |
| Magnesia | 2·20 | |
| Silica | 29·88 | } 42·58 |
| Alumina and peroxide of iron | 12·70 | |
| Sulphate of lime | 1·58 | |

*Quebec Cement.*—At Quebec in Lower Canada, a limestone belong-
ing to the Inferior Silurian is quarried for the purpose of making
cement. The rock which yields it was discovered by Captain Bad-
deley of the Royal Engineers. It is an argillaceous, compact lime-
stone, destitute of fossils, and strongly coloured by carbonaceous
matter. The cement itself was yellow, and effervesced feebly with
acids; it lost, when heated, 11·6 per cent. water and carbonic acid.
It set at the end of twenty-six minutes. The resulting mass was not
so hard as that from the Thorold cement. Its resistance to traction
was about 0·49 kil. Its composition was as follows:—

| | | |
|---|---|---|
| Lime | 52·49 | |
| Magnesia | traces | |
| Silica | 27·40 | } 39·56 |
| Alumina and oxide of iron | 12·66 | |
| Sulphate of lime | 7·95 | |

Although the composition of this cement is not very different from that of the Thorold cement, it is nevertheless of inferior quality.

*Chasy Cement.*—The Geological Commission of Canada has presented a magnesian limestone belonging to the Inferior Silurian, which might serve to prepare a cement. This limestone, which extends over more than fifty miles, is argillaceous, compact, and homogeneous. Its colour is gray. Although containing magnesia, it produces rather a lively effervescence with acids. Its composition is—

| | |
|---|---|
| Carbonate of lime | 45·30 |
| Carbonate of magnesia | 12·77 |
| Argillaceous residue | 19·77 |
| Alumina and oxide of iron | 12·52 |
| Water and loss | 9·64 |

*Oneida Cement.*—This is prepared from an earthy dolomite, which is of a deep gray colour, effervesces slowly with hydrochloric acid, and loses by calcination 37·5 per cent. water and carbonic acid. It occurs in the Upper Silurian formation. Its composition is,—

| | | |
|---|---|---|
| Carbonate of lime | 39·91 | |
| Carbonate of magnesia | 34·15 | |
| Silica and argillaceous residue | 15·85 | 22·10 |
| Alumina and oxide of iron | 6·25 | |
| Water | 3·84 | |

*Magnesian Mortar.*—The Geological Commission of Canada presented a specimen of a very remarkable rock formed of carbonate of magnesia. It resembled a granular marble so closely that it might have been mistaken for one, although it contains only insignificant traces of carbonate of lime. It is besides ferriferous, and in certain places is coloured green by oxide of chromium. It contains also quartz, disseminated in white hyaline grains. The following analysis agrees with a previous examination by Mr. Hunt :—

| | |
|---|---|
| Carbonate of magnesia | 58·29 |
| Carbonate of iron | 9·06 |
| Carbonate of lime | traces |
| Quartz | 30·12 |
| Water and loss | 2·55 |

This carbonate of magnesia might be advantageously worked for the preparation of the magnesian mortar, not alterable by the action of sea-water, which has been recently proposed by M. J. B. Vicat.

## LXIII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 397.]

February 21, 1856.—The Lord Wrottesley, President, in the Chair.

THE following communication was read :—

"On the Thermal Effects of Fluids in Motion." By Professor William Thomson, F.R.S., and J. P. Joule, Esq., F.R.S.

A very great depression of temperature has been remarked by some observers when steam of high pressure issues from a small orifice into the open air. After the experiments we have made on the rush of air in similar circumstances, it could not be doubted that a great elevation of temperature of the issuing steam might be observed as well as the great depression usually supposed to be the only result. The method to obtain the entire thermal effect is obviously that which we have already employed in our experiments on permanently elastic fluids, viz. to transmit the steam through a porous material and to ascertain its temperature as it enters into and issues from the resisting medium. We have made a preliminary experiment of this kind which may be sufficiently interesting to place on record before proceeding to obtain more exact numerical results.

A short pipe an inch and a half diameter was screwed into an elbow pipe inserted into the top of a high pressure steam-boiler. A cotton plug placed in the short pipe had a fine wire of platina passed through it, the ends of which were connected with iron wires passing away to a sensitive galvanometer. The deflection due to a given difference of temperature of the same metallic junctions having been previously ascertained, we were able to estimate the difference of temperature of the steam at the opposite ends of the plug. The result of several experiments showed that for each lb. of pressure by which the steam on the pressure side exceeded that of the atmophere on the exit side there was a cooling effect of 0·2 Cent. The steam, therefore, issued at a temperature above 100° Cent., and, consequently, *dry* ; showing the correctness of the view which we brought forward some years ago * as to the non-scalding property of steam issuing from a high pressure boiler.

February 28.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

The following Letter was read, from Professor Hansteen of Christiania, For. Mem. R.S. :—

#### *To the Royal Society of London.*

As a Corresponding Member of the Royal Society, I have the honour herewith to transmit a Research "On the Secular Changes of the Magnetical System of the Earth, and more specially on the Secular Variation of the Magnetical Inclination in the Northern

* See letter from Mr. Thomson to Mr. Joule, published in the Philosophical Magazine, Nov. 1850.

Temperate Zone," separately printed from the 'Memoirs of the Roy. Soc. of Sciences of Copenhagen.' By calculating newer and more ancient observations of the magnetical declination, I have ascertained the movement of the four magnetical polar regions, which I had already found in my work 'Untersuchungen über den Magnetismus der Erde' (Christiania, 1819, with Atlas); whereof the two northern ones have a motion from west to east, the two southern ones in the contrary direction; and have attempted thereby in general to declare the cause of the known variations, as well of the system of declination as of that of inclination and of intensity.

As I am indebted for the greatest part of the materials to English observations, I have found it my duty to render my thanks to English science, and to express my hopes of future exertions towards the solution of this, in my thought, most interesting problem of the general physics of the globe.

<div align="center">Most respectfully,</div>

<div align="right">CHRISTOPHER HANSTEEN.</div>

Observatory in Christiania,
December 31, 1855.

March 6.—Colonel Sabine, R.A., V.P. and Treasurer, in the Chair.

The following communications were read:—

"Supplement to the 'Account of Pendulum Experiments undertaken in the Harton Colliery;' being an Account of Experiments undertaken to determine the correction for the Temperature of the Pendulum." By G. B. Airy, Esq., Astronomer Royal.

Adverting to the circumstance that, in the Harton Experiment, there was a mean difference of $7°$ between the temperature above and below, and that a careful determination of the coefficient for temperature-correction was therefore necessary, the author describes the process by which the correction was now investigated by experiment on the same pendulums which were used in the Harton Experiment. Two rooms were selected at the Royal Observatory, Greenwich, having firm stone floors, and admitting of being heated, one by a stove in the room, the other by a hot-air-apparatus below. One pendulum was mounted upon its iron stand, with clock and other apparatus, in one room, and the other in the other room. Care was taken that the pendulums and their thermometers should be effectually protected from radiation. The two clocks were compared by carrying a chronometer from one to the other, and remarking the time of coincidence of beats; a method which admits of very great accuracy, when (as in this instance) the distance through which the chronometer is to be carried is small. In the Fifth Series (counting the series in sequence to those of the Harton Experiment), Pendulum 1821 was kept in heat, and Pendulum 8 cool, and continuous observations were kept up during forty hours. In the Sixth Series, Pendulum 8 only was kept in heat, and observations were again kept up during forty hours. The Seventh and Eighth Series were similar, respectively, to the Fifth and Sixth. The temperatures are referred to two of the thermometers used in the Harton Experiment, and to two other thermometers supplying the place of two of the Harton

thermometers which cannot be found.    The observations were con-
ducted entirely by Messrs. Dunkin and Ellis, Assistants of the Royal
Observatory.

On discussing the results of the observations, there appears to be
reason for supposing that a change has taken place in one of the
pendulums after the Seventh Series.    This appears from the circum-
stance that, though the Fifth and Seventh Series agree well, the Sixth
and Eighth are discordant ; and also from this circumstance, that the
abstract relation between the two pendulums given by the Fifth,
Sixth, and Seventh Series, agrees closely with that found at Harton ;
but if the Eighth Series is included, there is a considerable dis-
cordance.

If the Eighth Series is rejected, it appears that Colonel Sabine's
coefficient ought to be increased by about $\frac{1}{28}$th part ; and on intro-
ducing this correction into the computations of the Harton Experi-
ment, the result for the earth's mean density is 6·809.    If the
Eighth Series is retained, the correction is reduced to less than one-
fourth of that just mentioned, and the earth's mean density is
6·623.

The author subjoins an investigation with which he has been
favoured by Professor Stokes on the effect of the rotation and ellip-
ticity of the earth in modifying the numerical results of the Harton
Experiment.    It appears that the numbers found in the paper ought
to be multiplied by

$$1 + m - \frac{\epsilon}{2} + \frac{3}{2}\epsilon\cos 2l,$$

where $m = \dfrac{\text{equatoreal centrifugal force}}{\text{gravity}}$

$\epsilon = $ ellipticity

$l = $ latitude of place.

On converting this formula into numbers, for Harton, the factor is
found to be 1·00012, which produces no sensible change in the
result.

At the equator the factor would have been 1·00679.

"On the Mathematical Theory of the Stability of Earthwork and
Masonry."    In a Letter to Prof. Stokes, Sec. R.S.    By W. J. Mac-
quorn Rankine, Esq., C.E., F.R.S.

In the preparation of my course of lectures, I have found it neces-
sary to re-investigate much of the above-named branch of mechanics,
and I have now a paper in preparation on the subject, which I pro-
pose to offer to the Royal Society when it is ready.    In the mean-
while, it appears to me that the two fundamental principles on which
my researches are based are of such a nature, that they may very
properly be communicated to the Royal Society at once.    They are
as follows :—

### I. *Principle of the Stability of Earth.*

At each point in a mass of earth the directions of greatest and
least compressive stress are at right angles to each other ; and the

condition of stability is, that at each point the ratio of the difference of those stresses to their sum shall not exceed the sine of the angle of natural slope of the earth.

## II. *Principle of the Transformation of Structures.*

Let a structure of a given uniform transverse section be stable under a system of forces represented by given lines in the plane of section :—Then will any other structure whose transverse section is a *projection by parallel lines* of that of the first structure upon any other plane, be stable under the system of forces represented by the projections, upon the new plane, of the lines representing the first system of forces.

### *Example of the application of this principle.*

Let fig. 1 be an equilibrated arch with its abutments of the form (for example) proposed by M. Yvon-Villarceaux, suited for a horizontal extrados EF. OK, OA, and AB being given, all the dimensions of the arch and abutments are functions of those three quantities.

It is required to design an arch, fig. 2, for an extrados *e f,* at any given inclination, of any given span *cd* (measured parallel to the extrados), and in which *ok*=OK, *oa*=OA, and *ab*=AB, are the same as in the primitive arch fig. 1.

Fig. 1.
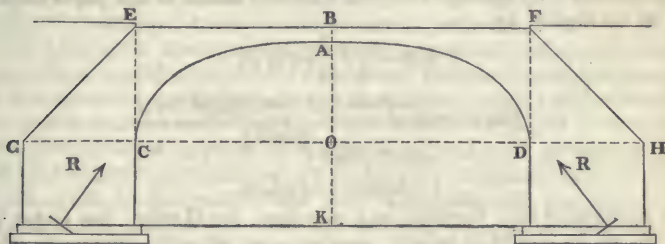


Fig. 2.
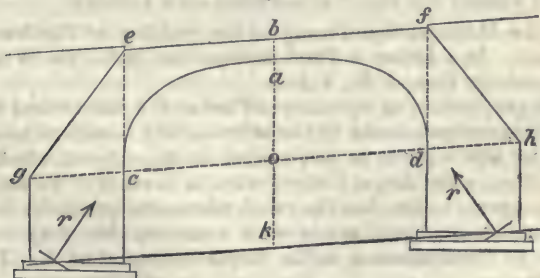


*Solution.* On any vertical plane passing through BK, and not coinciding with the plane of fig. 1, draw *cod* of the given length and inclination, intersecting COD in O. Join C*c*, D*d*, and project the

whole of fig. 1 on the new plane by lines parallel to C*c*, D*d*. The projection so obtained will be the figure of the arch and abutments required. Moreover, if the lines R, R, fig. 1, represent in length, direction, and position, the resultants of the pressures of the abutments on their foundations in the original arch, then will $r, r$, fig. 2, the projections of R,. R, represent the corresponding resultants in the new arch; and in like manner, the thrust at $a$ is the projection of the thrust at A.

Glasgow, 18th February, 1856.　　　　W. J. MACQUORN RANKINE.

*Note*. The horizontal foundation courses in fig. 2 do not form part of the projection of fig. 1, but are supposed to be added after the completion of the projection.

March 13.—Sir Benjamin C. Brodie, Bart., V.P., in the Chair.

The following communication was read :—

"On the presence of fibrils of soft tissue in the Dentinal Tubes." By John Tomes, Esq., F.R.S.

Referring to the structural characters of dentine, and to the prevailing belief that the dentinal tubes in the normal condition contain fluid, the author goes on to show that the recognized histological characters fail to account for the high degree of sensibility exhibited by the dentine when diseased, or when suddenly exposed by the removal of the enamel.

It is found, moreover, that the dentine is not uniformly sensitive throughout, but possesses a much higher degree of sensibility at the peripheral distribution of the dentinal tubes than deeper in the substance of the tooth; and it is urged that these facts cannot be accounted for by the presence of a fluid in the dentinal tubes, nor by supposing that the hard unyielding dentine is intrinsically endowed with sensation. This view of the matter is borne out by the fact, that all sensibility is at once lost if the pulp of the tooth be destroyed.

Finding that the dentine owed its sensibility to the presence of the dentinal pulp, and knowing that the tubes have open extremities in contact with the pulp, the author was induced to examine carefully the contents of the tubes. The investigation resulted in discovering that the dentinal tubes, instead of containing fluid only, give passage to fibrils of soft tissue, which pass from the pulp into the tubes where these open upon the surface of the pulp-cavity, and from thence may be traced into the branches. The fibrils may be demonstrated by fracturing a perfectly fresh tooth, and then with a sharp knife taking very thin sections from the dentine near the edge of the pulp-cavity. The dentine will, when cut, break up into small fragments, and from the edges of these the fibrils may be seen extending. Sometimes a small portion of the pulp will be found adherent, in which case the fibrils may be seen to extend from that tissue into the dentine. The fibrils may be shown in a more striking manner by decalcifying a section, and then, when it is placed upon a slide, tearing the specimen across the direction of the tubes. By

this manipulation, the fibrils will be dragged out from one fragment, and will be seen projecting from the edge of the other.

The fibrils, when isolated and examined with a high power, without the presence of a reagent, show some indications of tubularity, but not with sufficient distinctness to enable the author to determine whether they are tubes or solid bodies. Their appearance is very like that of the ultimate fibrils of spinal nerves, and they possess a character in common with these, in the presence of minute globules of dense transparent matter exuded from the broken ends, and sometimes from the surface of the fibril. It is not easy to determine in what manner the fibrils commence in the pulp. In some preparations they appear to be connected with cells situated a short distance within the pulp, in others they may be traced to a greater depth, where they are lost in the tissue of the pulp, and may possibly be connected with the nerves, which in this part are very abundant. But in the absence of exact knowledge as to the manner in which the dentinal fibrils are related to the elements of the pulp, the author considers that there is sufficient evidence to warrant the conclusion that they are organs of sensation, the distribution of which through the substance of the dentine endows that tissue with its sensibility.

This conclusion is borne out by the occurrence of the following conditions. If a fragment of enamel be broken from the surface of the dentine, the exposed portion of the latter tissue is highly sensitive to the contact of foreign bodies; but if the force producing the injury be sufficient to rupture the nerves and vessels where they enter the root of the tooth, the dentine loses its capability of feeling pain. Again, if the dentine be exposed by the gradual wearing away of the enamel by mastication, the surface evinces no sensibility, a circumstance accounted for by the fact that the dentinal tubes have become consolidated, either at the surface exposed, or at some point between the surface and the pulp-cavity. Diseased teeth furnish further evidence in favour of the foregoing views. If a carious tooth, in which the disease has advanced but slowly, and the carious portion is dark in colour and tolerably firm in consistence, be examined, it will be found that the dentinal fibrils have become calcified, and in a favourable section they may be seen projecting from the edge of the specimen, or lying broken in short lengths in the tubes. On removing the diseased part of the tooth in such a case, no pain is experienced until the instrument reaches the healthy dentine. Supposing, however, the disease to have been rapid in its progress, the carious tissue will be light in colour, and, as compared with the preceding example, soft in consistence. The removal of the affected part in this case is frequently attended with considerable pain. Examination will then show that the dentinal fibrils have not been consolidated, but may be found here and there extending into the softened tissue without having suffered any appreciable alteration of appearance.

Daily experience shows that a tooth may remain useful for a long time after the pulp, and consequently the dentinal fibrils, have been destroyed. If, however, a tooth which has been so circumstanced be

examined, it will be found that one of two actions has been set up. Either additional cementum will have been developed upon the surface of the fang, or its bulk will have become diminished by absorption. Similar conditions supervene when the crown of a tooth has been lost by caries.

In old persons we find the teeth are lost without apparent disease in the dental tissues. The teeth become loose and fall out, the roots being in such cases translucent like horn. This condition is the result of consolidation of the dentinal fibrils, and is followed by absorption of the cementum and dentine. Cases may be found in which the whole of the fang has been absorbed, but reduction to two-thirds or half of the normal bulk is very common.

The concurrence of the foregoing changes in the sensibility of the tooth, with the destruction or consolidation of the dentinal fibrils, will, the author considers, justify the conclusion, that the dentinal fibrils, in a state of integrity, are necessary to the normal condition of dentine.

April 10.—Col. Sabine, R.A., V.P. and Treasurer, in the Chair.

The following communications were read :—

"Account of 'Experiments on the Vagus and Spinal Accessory Nerves." By Augustus Waller, M.D., F.R.S.

The important functions of the organs more or less completely dependent for their innervation on the vagus nerve, have afforded the reason of so many attempts by previous physiologists to determine the exact influence exerted by the fibres arising from different sources which are intimately blended together in the trunk of the mixed vagus. Since Sir Charles Bell's discovery of the different functions of the anterior and posterior roots of the spinal pairs, it has become still more important to determine how far the same law holds good with regard to the vagus nerve, and whether at its origin, it is a purely sensory nerve, receiving its motor fibres from the internal branch of the spinal accessory and perhaps from other sources. According to Bischoff and Longet, the vagus at its origin and as far as the upper ganglion, is purely sensory, and becomes possessed of motor power from its junction with the internal branch of the accessory, and from other branches derived from motor nerves (Longet). The observations of Bernard have led him on the contrary to adopt the opinion, that the vagus at its origin is a mixed nerve ; because after destroying the spinal accessory, no effect on the functions of the heart, stomach, or lungs was observed, and the only organs visibly dependent on the spinal accessory were the larynx and pharynx. After bearing testimony to the correctness of the observations made by Bernard with regard to the effects immediately following destruction of the spinal accessory, the author considered it desirable to apply to the determination of this question his new means of investigation, in addition to those previously employed. His first observations on the vagus, published in the 'Comptes Rendus' of the Académie des Sciences, 1852, had already led him to entertain the idea, that the vagus proper was a purely sensory nerve. They first consisted in cutting this nerve

between its upper and lower ganglia, the section comprising the internal branch of the spinal accessory. After the animal had been kept alive for a sufficient period to cause disorganization of the nerve fibres, the central and distal portions were examined microscopically. In the central portion, the fibres of the internal branch of the spinal accessory included in the section were all in a sound state; some of the fascicles of origin of the vagus were also sound; the remaining fascicles of origin of the vagus consisted of disorganized fibres. The analogy existing between the results as to the last-mentioned fibres, and those which follow section of the sensory roots of the spinal nerves, led the author to the conclusion, that the disorganized fibres of the fascicles of the vagus were likewise sensory, and had their trophic centre in its lower ganglion, while the sound fibres probably had their trophic centre in the upper ganglion. On these grounds, and some others affording concurrent testimony, he concluded that the vagus in itself was probably a purely sensory nerve. In the distal end, the part below the second ganglion consisted likewise of a mixture of sound and altered fibres. When tested by galvanism on the living animal, it was found that the nerve had lost all power of exciting movement in the various organs which it supplies, and that such branches of known motor power as the recurrent and the crico-thyroid were disorganized in their structure. It was evident therefore that the lower ganglion did not arrest the disorganization of the motor fibres contained in the vagus, because, as has been proved by the author in other cases, when the disorganization of the motor fibres of nerves distributed to muscles is arrested,—as by the superior cervical ganglion of the sympathetic nerve, or by the effect of hybernation in the frog,— the motor power of these nerves is retained.

In the present observations, the accessory nerve was divided at its origin by Bernard's process of evulsion of its roots, and after the lapse of about a fortnight the vagus was tested by galvanism, and examined by the microscope, in order to ascertain the functions which it had lost, and the fibres which were disorganized, the sound vagus on the opposite side being taken as a standard of comparison. On the sound side, galvanism of the vagus caused each time strong dilatation of the glottis by the retraction of the corresponding arytenoid cartilage. On the operated side, galvanism produced a slight movement of the glottis on the same side by drawing inwards of the arytenoid.

On the heart, the action of galvanism of the sound vagus was manifest by the stoppage of the pulsation of the carotid arteries and the diminution of their calibre. On the operated side, no influence on the pulsation of the vessels was observed by galvanizing the vagus. When the heart was exposed by removing a portion of the thorax, and keeping up artificial respiration, galvanism on the sound vagus produced complete stoppage of the heart's action, while on the opposite side, irritation of the vagus exerted no influence on the heart.

The stomach being exposed was found distended with food. Galvanism of the sound vagus caused evident contractions of this organ, which were strongest at the neck or constriction which it usually presents (in the rabbit),—from whence they radiated in both directions, becoming more and more faint. On stimulating the other vagus, from which the accessory had been virtually eliminated, no perceptible influence was observed.

Microscopic examination showed that the cervical part of the vagus of the side operated on contained numerous disorganized fibres, almost all collected together in a single fasciculus, which included only a few normal fibres. In the recurrent branch were disorganized fibres, corresponding very closely to those found in the vagus above it. Below the recurrent, the vagus and its cardiac, pulmonary and gastric branches consisted almost entirely of normal fibres, most of which, as is well known, are nucleated fibres.

From the foregoing observations, the author draws the conclusion, that from the spinal accessory are derived the greater part of the motor fibres contained in the vagus, which govern the movement of the larynx, the heart, and the stomach. He likewise infers from the microscopic examination of the vagus below the recurrent, that the motor fibres distributed to the heart and stomach belong almost exclusively to the nucleated or 'Remak' fibres.

The author purposes communicating in a future paper his researches on the other organs supplied by the vagus.

The above experiments were principally carried out in the laboratory of M. Flourens at the Jardin des Plants, who facilitated in every way the researches, and where the author had the able assistance of Drs. Philipeaux and Vulpian.

"Extract of a Letter to George Rennie, Esq., F.R.S., from P. A. Secchi, Director of the Astronomical Observatory of the Collegio Romano, containing explanatory remarks on a drawing of the Lunar Spot 'Copernicus,' presented by him to the Royal Society." Dated Rome, March 13, 1856.

"As to the drawing of the spot of the moon, it is a first attempt to obtain an accurate representation of the interesting spot 'Copernicus.' In such large dimensions, photography directly taken with the telescope has been impossible; I therefore made first an accurate triangulation of the spot with the micrometer, and the principal points were thus laid down on the chart, after which operation the rest was filled in by the eye alone. The power used has been always either 1000 or 760. As it was impossible to carry through such a work in a single night, on the first night of good opportunity a general outline was taken, and on the other evenings particular drawings were made, and all these parts, taken in different grades of light and shadow, were afterwards harmonized together and compared with the moon when the point of light was seen to be the same as on the first night. So this work occupied more than six months, that is, all the favourable positions (two at each lunation) which could be obtained. I do not pretend it to be yet accurate

enough to be transferred from photography* to any kind of engraving, but I am watching every good occasion to make it complete. But before bestowing more time and labour, I should be glad to know the impression such a work may make among the scientific men of England. I must observe that the most distant outliers of the crater have not been included," &c.

"Notes on the Drawing of 'Copernicus,' presented to the Royal Society by P. A. Secchi." By John Phillips, Esq., F.R.S.

Of the few attempts which have been made of late years to prepare drawings†, on a large scale, of selected lunar mountains, this contribution from the Roman Observatory appears to be one of the most successful. It is on a scale of magnitude (about 10 geographical miles to one inch) such as only the larger modern telescopes can command, and characterized by such firmness of definition as to do honour alike to the maker of the instrument and to the artist engaged in the delineation. It may assist those who have not attempted, with their own hands, any drawings of this kind, and desire to form a right judgement of the value of this work of P. Secchi, if I send for comparison a drawing of Gassendi, executed from my object-glass of $6\frac{1}{4}$ inches (Cook), with a focal length of 11 feet. The drawing is on a scale of 20 geographical miles to an inch, and Gassendi thus appears of half the linear dimension of Copernicus, being really almost of the same diameter.

Placing together the two drawings, and remembering the appearance of Copernicus, as I have seen it through telescopes, some reflections arise which it may be permitted me to express, in the hope that we are now fairly entered on the long career of discoveries in the moon, to which the attention of astronomers has been of late systematically drawn by the Earl of Rosse and a Committee of the British Association.

In proportion as the power of the telescope rises, the seemingly simple 'ring mountains' of the moon exhibit as much diversity of outline and structure as the larger terrestrial volcanoes when accurately mapped. Thus while Gassendi,—40° from the central meridian of the moon, and 17° south‡ of the equator,—has the obliquely elliptical contour due to a circle in that position, Copernicus, 20° from the central meridian, and 10° north of the equator, has its most conspicuous peripheral crest formed of seven principal nearly straight elements, approaching to equality in length, and meeting in points which are situated almost exactly in a circle of 24 geographical miles radius. Here is a very important partial difference, coupled with a very important general agreement.

While Gassendi, with peaks 9000 feet high, projects like a huge narrow wall into the Mare Humorum, and hangs over the interior plain in precipices as steep and many times as high as those over the

* The figure presented to the Society is a photographic copy of the original drawing.—(Ed.)

† As distinct from mere *plans*. The drawings must however be based on exact plans.

‡ The Poles being named after the type of Mädler's noble work, ' Der Mond.'

Atrio del Cavallo, Copernicus, seated in the midst of broad land, on a base of 120 geographical miles, rises in many broken stages, bristling with a thousand silver-bright crests,—a perfect network of rough and complicated ground, crossed by lights and shades, which have a history of their own,—and toward the inside falls off by many irregular terraces, down to an interior plain, as if the whole area had yielded, and the surface had been formed by enormous land-slips. Four sharp notches are traced across the narrow ridge of Gassendi, cutting it deeply, like the hollows left by decomposing lava dykes 500 feet broad; one deeper and broader opening unites the inner plain with the outer Mare Humorum, and one far wider opening leads to an accessory crater, over whose awful depth the cliffs, 10,000 to 12,000 ft. high, spread black shadows round some central rocks. In these particulars Copernicus offers a very different aspect. Its high crest, of 10,000 feet, is only cut through by one straight narrow meridional groove, though broken by numerous fissures in other parts, and is in all parts so irregular, partially undulated, and varied with small crateriform points, and enclosed areas, resembling craters, as to offer little analogy to any truncated cone of eruption. The highest summit, on the left-hand (west) side—a huge rock—is conspicuous by its broad, deep and extended shade. What suggests a vast lava current, is equally remarkable on the northern slope. Regarding now the central plains of these mountains, we remark in each several low ridges of rather sinuous forms, and several small mounds (half a mile or more across), of which *three central digitated masses, not pierced by craters*, are the most elevated, and catch the earliest lights of morning which glance over the rocky borders of the basin. Had the drawings been executed at the instant of sunrise on the central meridian line of the basin, these points would have stood up on the soft edge of the light and shade, as bright as the Swiss mountains at sunrise or sunset, but not like them reddened by the optical property of the atmosphere. Gassendi has *at least two* (I have somewhere a memorandum of more) small craters within the central plain. None such appear in this drawing of Copernicus. In many other lunar mountains the centre is occupied by a crater-formed hill, as Vesuvius stands within Somma; in others the hill remains a smooth rounded mass, but its crater is lost; and a further stage of decay seems to be seen in Gassendi and Copernicus, where the central mass is broken into fragments and sculptured by ramified hollows. May we ascribe these effects to the former action of a lunar atmosphere, now absorbed in the oxidated crust of the moon? If so, the lunar mountains have a history of water, as well as records of fire, and we must look on the sinuous ridges of the Mare Humorum with eyes accustomed to the gravel mounds of Norway and Ireland; study the degraded craters after the models of the Eifel; and map the 'rillen *' with reference to valleys of erosion as well as of eruption.

In questions of this kind we shall find such drawings as this of the Roman astronomer of priceless value. Studied, scrutinized,

* I have some curious results regarding these beautiful objects.

enriched with new discoveries, it may be the model for all time to
come in this line of research. It may be followed by two other
drawings of the same mountain,—one at the moment when the sun
is on the meridian of the central hillocks, *to show the light streaks,*
which hide themselves when the sun is low, and another in the
clear afternoon of the lunar day (as much after midday, as this
drawing was taken before noon), when every little crack and cavity
becomes again distinct, *but greatly altered in aspect,* and the whole
landscape changes under the eye of the observer; the plains grow-
ing grayer and softer, and revealing many minute low undulations;
the hills looking more and more rugged, and burning with narrower,
brighter and more angular tracts of silvery light.

"A Third Memoir on Quantics." By Arthur Cayley, Esq., F.R.S.
The object of the author in the present memoir is chiefly to col-
lect together and put upon record various results useful in the theo-
ries of the particular quantics to which they relate. The tables at the
commencement relate to binary quantics, and are a direct sequel to
the tables in the author's second memoir upon Quantics, Phil. Trans.
vol. cxlvi. (1856). The definitions and explanations in the next
part of the present memoir are given here for the sake of conve-
nience, the further development of the subjects to which they relate
being reserved for another occasion. The remainder of the memoir
consists of tables and explanations relating to ternary quadrics and
cubics.

"Elementary Considerations on the subject of Rotatory Motion."
By W. Gravatt, Esq., F.R.S.
The author explains the subject of rotatory motion in a series of
propositions by the use of prime and ultimate ratios. He com-
mences with a simple problem, determining the law of the forces by
which a particle of matter is deflected into any given course, and
pursues the inquiry by a consideration of the effect of these forces as
referred to a sphere, going on to the investigation of the character of
the motion of any body enclosed within an imaginary sphere, such
sphere itself being supposed to revolve upon two axes inclined at any
angle to each other. Hence the author determines the position of
some point of the circumscribing sphere momentarily at rest, or in
other words, of the *resultant axis,* from which he insists that all
centrifugal forces must really be calculated.

His first application of the law thus enunciated is to the motion of
the peg-top; and upon the principles he has already laid down, he
shows that there is in the first instance rotation round a momentary
horizontal axis, calling up rotation round a momentary vertical axis;
and that the ratio of the velocities of these two rotations, together
with the length of the peg, determines the angular inclination of the
top, contrary to the received explanation as given by Euler and other
mathematicians.

The law is further applied to the effect produced upon a falling
body by the axial rotation of the earth, in the discussion of which,
La Place, in the opinion of the author, has committed two important

errors; one in denying any deviation towards the equator, the other in his calculation of the amount of the deviation towards the east.

This is followed by an investigation of the motion or direction of flight of a cannon-ball or shell fired in a northerly or southerly direction, from which it appears that a large shell will be subject to a deviation from the true line of projection, in consequence of the earth's rotation, amounting to no less than 22 feet.

The author then refers to the well-known experiment of M. Foucault for proving sensibly the rotation of the earth, and shows from calculation that the errors which would be sufficient to vitiate the results in this experiment are so extremely minute and so difficult of avoidance by any perfection of manipulation which can be employed, that its performance cannot perhaps be safely adduced as proving such rotation.

The author illustrated his views by the exhibition to the meeting of a model apparatus, in which the vertical and horizontal motions may be variously combined, but which could not be intelligibly described without a series of complicated drawings unfitted for the compass of a mere abstract.

April 17.—The Lord Wrottesley, President, in the Chair.

The following communication was read :—

"On the Condition of the Oxygen absorbed into the Blood during Respiration." By George Harley, M.D.

The author commences by explaining, that his researches were instituted with the view of ascertaining whether the doctrine maintained by Magnus in regard to the gases interchanged in the lungs during respiration were correct—namely, that the gases in question enter into *no* chemical combination with the constituents of the blood, either in passing to or from the tissues and organs of the body, but form merely a physical mixture with the circulating liquid. The principal object of the inquiry was to determine the following points :—

1. Has blood the property of chemically combining with the respired oxygen?

2. Which of the constituents of the blood enter into combination with oxygen?

3. Do these constituents, by combining with oxygen, simply become oxidized, or do they also yield carbonic acid gas?

4. What are the agents which control these changes?

After describing the method of investigation, and the apparatus employed, the author proceeds to relate a few of the analyses which he considered as the most conclusive. Instead of confirming the view of Magnus, that gases enter into no chemical combination with blood, his results led him to conclusions of an opposite character, which serve to confirm the more generally received doctrine.

In one set of experiments a certain quantity of fresh ox-blood was first shaken with renewed portions of air until it had become thoroughly saturated with oxygen, then introduced into a graduated glass vessel with 100 per cent. of ordinary air, corked carefully up,

and kept during twenty-four hours in a room of moderate temperature. In order to favour the mutual action of the air and blood, the vessel was frequently agitated. At the expiration of twenty-four hours the gas was analysed by Bunsen's method. In an example cited the following was found to be its composition :—

$$\left. \begin{array}{l} \text{Oxygen.} \dots \dots \dots 10\cdot42 \\ \text{Carbonic acid} \ \dots\ 5\cdot05 \end{array} \right\} \text{total oxygen} = 15\cdot47$$
$$\text{Nitrogen} \ \dots\dots\dots 84\cdot53$$

$$\overline{\phantom{xxxx} 100\cdot00 \phantom{xxxx}}$$

On comparing this with the composition of the common air (oxygen $20\cdot96$; carbonic acid $00\cdot002$; nitrogen $79\cdot038$) which had been introduced into the vessel, it is seen that $10\cdot54$ per cent. of oxygen has disappeared, while $5\cdot05$ per cent. of carbonic acid now exists, where only a trace of its presence could before be detected.

Similar results were obtained with defibrinated blood. In a case where defibrinated arterial blood from a calf, after complete saturation with oxygen, was kept in contact with an equal volume of air during twenty-four hours, and treated exactly as in the previous example, the gas on analysis yielded in 100 parts,—

$$\left. \begin{array}{l} \text{Oxygen.} \dots \dots \dots \ 11\cdot33 \\ \text{Carbonic acid} \ \dots\ 5\cdot96 \end{array} \right\} \text{total oxygen} = 17\cdot29$$
$$\text{Nitrogen} \ \dots\dots\dots 82\cdot71$$

$$\overline{\phantom{xxxx} 100\cdot00 \phantom{xxxx}}$$

showing in this case also that the air which had been imprisoned during twenty-four hours along with blood, no longer possessed its original composition, but that some of its constituents had been materially increased, while others had diminished in a manner no less marked.

It would appear from these examples that the blood had probably become oxidized in two ways; first, by giving off a quantity of carbon, and secondly by directly combining with oxygen. As to the portion of oxygen which has disappeared, and which is not accounted for by the carbonic acid evolved, it may have combined partly with another portion of carbon, to form a limited amount of carbonic acid, which by the law of absorption is retained in the blood; and partly with hydrogen or some other oxidable constituent of the blood, without yielding a gaseous product.

These two experiments it will be observed point to exactly the same conclusions, and together with a number of others, where the mode of procedure was similar, and which were attended with similar results, have satisfied the author as to the fallacy of Magnus's doctrine, " that the oxygen received during respiration into the blood is kept there merely by the law of mechanical absorption, and enters into no chemical combination with that liquid." Had this assertion been well-founded, such a change as has been seen to occur, in the

composition of the air enclosed along with blood, saturated as the
blood already was with oxygen, could not have happened.

After having ascertained that air underwent certain changes in
composition during its contact with blood, it next became an object
to discover by which of the constituents of the blood these changes
were induced. With this view the author successively subjected the
organic compounds of the blood separately to the action of air, by
a similar process to that adopted in the case of the blood itself.

A certain quantity of fresh fibrine, moistened with water, was satu-
rated with oxygen, placed in a receiver along with eight volumes of
air, and kept during twenty-four hours at a temperature of from
20° to 25° Cent. At the expiration of this time the gas on analysis
was found to have the following composition :—

$$\left.\begin{array}{l}\text{Oxygen.......... } 6\text{·}81 \\ \text{Carbonic acid .... } 11\text{·}17 \\ \text{Nitrogen ........}82\text{·}02\end{array}\right\} \text{total oxygen} = 17\text{·}98$$

$$\overline{100\text{·}00}$$

thus showing that fibrine takes up a certain quantity of oxygen, and
gives off a stated amount of carbon combined with oxygen in form
of carbonic acid gas.

The next experiments were made upon albumen, but as that sub-
stance could not be obtained in a pure, and at the same time un-
coagulated state from blood, the albumen of the hen's egg was
employed, which possesses very similar characters. It was found
that when a certain quantity of the white of the hen's egg was well
saturated with oxygen, and afterwards kept in contact with an equal
volume of air during a certain number of hours at a temperature of
36° Cent., the gas on analysis gave in 100 parts,—

$$\begin{array}{l}\text{Oxygen ...................,} 17\text{·}05 \\ \text{Carbonic acid................} 2\text{·}09 \\ \text{Nitrogen ..................}80\text{·}86\end{array}$$

$$\overline{100\text{·}00}$$

proving, in common with the experiments on the blood and on
fibrine, that albumen also possesses the property of absorbing oxygen
and disengaging carbonic acid.

Some comparative experiments were also made upon serum and
upon blood-coagulum, in which it was found that the air confined
along with the serum yielded on analysis—

$$\begin{array}{l}\text{Oxygen ...................} 16\text{·}74 \\ \text{Carbonic acid................} 2\text{·}30 \\ \text{Nitrogen ..................}80\text{·}96\end{array}$$

$$\overline{100\text{·}00}$$

while that confined with the coagulum contained—

Oxygen .................... 8·57
Carbonic acid................ 7·29
Nitrogen ................. 84·14
                              ———
                           100·00

It thus appears that the oxygen exerted a much more powerful action on the coagulum, which contained the fibrine and blood-corpuscles, than on the serum, which contained only albumen. The experiment thus corroborated the results previously obtained with pure fibrine and pure albumen. The pure fibrine was seen to produce a much greater change in the composition of the atmospheric air than the pure albumen from the hen's egg. The difference in the case of the coagulum and the serum was so much marked, that the author felt anxious to find out whence it proceeded; and under the impression that the hæmatine in the corpuscles might have mainly contributed to the result (as other organic colouring matters possess the property of absorbing oxygen and giving off carbonic acid gas), he took a small quantity of pure blood-hæmatine prepared by Verdeil's process, and put it into a vessel along with 1000 volumes of ordinary air. After the air had been kept in contact with the hæmatine for some months, the gas was analysed and found to contain—

Oxygen .................... 16·01
Carbonic acid................ 3·80
Nitrogen ................. 80·19
                              ———
                           100·00

The pure colouring principle of the blood, therefore, by exposure to ordinary air, gives off carbonic acid gas, and becomes oxidized in two ways; first by a loss of carbon, and secondly by direct combination with oxygen. The author considers that this last result furnishes additional evidence of the correctness of an opinion he hazarded two years ago[*], imputing to the colouring matters of the vegetable and animal economy a more important office in the function of respiration than they before had been considered to possess, and regarding their principal function in organized beings as the absorbing of oxygen and exhaling of carbonic acid—a view altogether irrespective of Liebig's well-known hypothesis, which assigns the above office to the iron of the blood-hæmatine.

The author concludes by expressing the hope that his experiments will be considered as at least serving to establish one important fact respecting which further evidence was wanted, namely, that the entire volume of the respired oxygen is not transmitted in an uncombined state (as Magnus believes) to the various organs and tissues of the body, but that a portion of it enters into chemical combination with some of the organic constituents of the blood.

[*] Verhand. Physik-Medizin. Gesellsch. zu Würzburg, Bd. v. 1854; and Erdmann's Journ. f. prakt. Chemie, Bd. lxiv. H. 5. 1855.

[Continued from p. 165.]

November 5, 1856.—Colonel Portlock, President, in the Chair.

The following communication was read :—

" On the *Stereognathus Ooliticus*, from the Stonesfield-slate."
By Prof. Owen, F.R.S., F.G.S.

The subject of this paper was a small mammal, represented by a
fragment of a lower jaw retaining three molar teeth, which was ob-
tained by the Rev. J. Dennis from the Stonesfield-slate of Oxford-
shire, and named *Stereognathus Ooliticus* by Mr. E. Charlesworth.
This specimen, described in detail by Prof. Owen at the British As-
sociation Meeting in September last, indicated, in the author's opi-
nion, an animal allied to some extinct genera of even-toed pachy-
derms, viz. the *Hyracotherium*, *Microtherium*, and *Hyopotamus* of the
Tertiary deposits ; and he concluded therefore that the *Stereognathus*
was most probably a diminutive non-ruminant Artiodactyle of omni-
vorous habits.

With regard to the zoological reasons for referring this peculiar
and ancient fossil to the type of animal form above alluded to, the
Professor entered at some length into the analysis of the mental pro-
cesses by which the palæontologist aims at the restoration of an
unknown mammal from such a fragment as the fossil under notice.
Its mammalian character is decided by the two-fanged implantation
of the teeth, and its pachydermatous affinities are evidenced by the
peculiar sex-cuspid and cingulated molars. These zoological rela-
tions are determined from the knowledge that such structural pecu-
liarities obtain in certain known pachydermata. Morphology, there-
fore, or the study of form, rather than physiology, or the known
relation of organs to function, is the guide in this determination ;
but the Professor expressed his opinion that this example could not
be cited as showing that there is no physiological, comprehensible,
or rational law (in contradistinction to the morphological or empiri-
cal) which can be a guide in the determination of fossil remains.
He did not think that all such determinations rest upon the applica-
tion of observed coincidences of structure, for which coincidences
no reason can be rendered ; for, although in many instances of this
law of correlation, as demonstrated by comparative anatomy, the
sufficient or physiological cause of them is not known ; yet, in
other instances, the application of the principle has been suc-
cessfully illustrated. The truth or fact (said Prof. Owen) of a phy-
siological knowledge of a correlated structure, and of the applica-
tion of that knowledge to palæontology, is not affected or de-
stroyed by instances adduced from that much more extensive series
of correlated structures of which the physiological condition is not
yet known.

## LXIV. *Intelligence and Miscellaneous Articles.*

NOTE ON THE OPTICAL PROPERTIES OF TRANSPARENT BODIES SUBMITTED TO THE ACTION OF MAGNETISM. BY M. VERDET.

MANY physicists have indicated the relations between the rotation of the plane of polarization produced under the influence of magnetism and various physical properties of transparent bodies. M. De la Rive, in the first volume of his *Traité de l'Electricité*, in referring to the experiments of M. Bertin, calls attention to the fact that the rotation is generally stronger in proportion as the index of refraction is greater. Two substances cited in the Table which M. Bertin has inserted in his memoir *, form exceptions to this rule; these are alcohol and æther, which are more refractive than water, and which, nevertheless, under the influence of magnetism, rotate the plane of polarization of light at an angle considerably less. In commencing my investigation I have had more particularly in view to determine the extent of application of De la Rive's rule, which various reasons, unnecessary to be reproduced here, led me to regard as well founded. I have consequently measured the index of refraction of a considerable number of substances, and I have then compared the action which they exert upon polarized light, when placed between the poles of an electro-magnet. So as only to employ well-defined bodies and such as can be easily obtained of equal thickness, I have operated exclusively upon liquids, and particularly upon saline solutions. My experiments on the whole have not been favourable to the rule which I was endeavouring to verify, and I think that I may conclude from them that there exists no relation between the index of refraction, and that which, for the sake of brevity, I may be allowed to call the *magnetic rotatory power*. The following Table contains the results of a certain number of experiments in which the rule proposed by De la Rive is shown to be very evidently in the wrong.

| Nature of the substance. | Average index of refraction. | Complete rotation † produced by a thickness of 44 mm. |
|---|---|---|
| Distilled water ...................... | 1·334 | 4° 00″ |
| Solution of muriate of ammonia (dilute) .... | 1·359 | 4 45 |
| ,, ,, protochloride of tin (dilute)...... | 1·364 | 5 27 |
| ,, ,, muriate of ammonia (concentrated) | 1·370 | 5 29 |
| ,, ,, carbonate of potash ............ | 1·371 | 4 21 |
| ,, ,, chloride of calcium ........... | 1·372 | 4 55 |
| ,, ,, protochloride of tin (dilute)...... | 1·378 | 6 10 |
| ,, ,, chloride of zinc............... | 1·394 | 5 57 |
| ,, ,, protochloride of tin (concentrated). | 1·424 | 8 16 |
| ,, ,, nitrate of ammonia ........... | 1·448 | 3 44 |
| Liquid chloride of carbon ($C^2 Cl^4$) .......... | 1·466 | 5 12 |

M. Bertin has ascertained that certain substances, namely, nitrate of ammonia and protosulphate of iron, when dissolved in water,

* *Annales de Chim. et de Phys.* 3rd series, vol. xxiii.

† I give the name of *complete rotation* to the difference of the two azimuths of the tint of passage corresponding to two opposite directions of the current.

diminish the magnetic rotatory power of the solution.  M. E. Becquerel has made an analogous observation upon the protochloride of iron, and he believed that it might be said in a general way, that the rotation of the plane of polarization due to the influence of magnetism varies in an opposite ratio to the magnetic power of the bodies.   The experiments referred to by M. E. Becquerel do not allow us to consider this law as absolute.  Thus we see by them, that the rotation of water being represented by 10, those of the two unequally concentrated solutions of protochloride of iron are represented by 9 and by 3, and that of a solution of sulphate of nickel by 13·55 ; in other words, of these magnetic solutions there are two which produce a weaker rotation than water, whilst the third produces a stronger one.   Nevertheless, the extreme weakness of the rotation of a concentrated solution of protochloride of iron, approaching the observation of M. Bertin on the protosulphate of iron, seems to indicate that there is a peculiar mode of action in the ferruginous compounds deserving of careful study.

I have dissolved in water a certain number of proto- and per-salts of iron (chlorides, sulphates, nitrates), and found that in all cases the rotatory power of the solution was less than that of water.  But what is more, if, taking into account the density and composition of the solution, we calculate the rotation that would be produced by the quantity of water alone which it contains in a given thickness, we find a number constantly superior to the rotation observed. Things take place therefore as though the dissolved iron salt possessed a rotatory power in the opposite direction to that of water.

I proposed to ascertain whether this hypothesis was the true explanation of the phænomena, and I think I have succeeded in proving it.   After numerous fruitless attempts to procure a solid or readily fusible ferruginous body, sufficiently transparent with a thickness of 1 to 2 centimetres, and not exerting of itself any action upon polarized light, I succeeded completely by dissolving the salts of iron in vehicles, such as alcohol and æther, capable of taking up a considerable quantity of the salt, and endowed with so weak a magnetic rotatory power, as to show the direction of the rotatory power of the dissolved compound.   Thus by mixing 8 grammes of anhydrous perchloride of iron with 32 grammes of rectified æther, I obtained a liquid of a strong red-brown colour, but perfectly limpid, which under the influence of magnetism deviated the plane of polarization to the left under circumstances in which water and other transparent substances deviate it to the right, and *vice versâ*.   With 32 grammes of æther and only 4 grammes of the perchloride, I obtained a liquid which, under the influence of the electro-magnet which I had at my disposal, scarcely exerted any action upon polarized light.   Alcoholic solutions gave me exactly similar results. Moreover, it is easy to ascertain that the ætherial or alcoholic solutions of alkaline or metallic salts behave in general like aqueous solutions.   It is therefore to the iron salt dissolved in æther or alcohol that we must attribute the remarkable phænomenon which I have just made known, and from this we must conclude that the

salts of iron submitted to the action of magnetism exert an action opposite to that of the generality of transparent substances upon polarized light.

I shall propose to call the magnetic rotatory power of water, heavy glass, sulphuret of carbon, and most transparent bodies *direct*, and that of the salts of iron *inverse*.

It was natural to inquire whether any other magnetic salts, besides those of iron, would not present analogous phænomena. I am not in a position to give a certain opinion except upon the salts of nickel and manganese; of these I have examined a certain number, such as the sulphate, nitrate, and chloride of nickel, and the sulphate and chloride of manganese; and I have ascertained that in solution they possess a direct rotatory power, which is added to that of the water. They present no difference therefore from the ordinary metallic salts. I can say nothing certain as to the salts of chrome and cobalt: these compounds have such a great colouring power that their solutions must be very dilute if we wish to have them sufficiently transparent; the influence of the dissolved salts is then very weak in comparison with that of the solvent, and I have been unable to ascertain positively its direction, the apparatus at my command not being sufficiently powerful. I need not point out the new difficulty which the opposition of the optical properties of the salts of iron and nickel presents to the establishment of any theory of the phænomena. At any rate it is impossible to say simply that the rotation of the plane of polarization is weak in proportion as the magnetic capacity is strong, since we find magnetic bodies presenting rotatory powers in opposite directions.

Lastly, I have examined the solution of nitrate of ammonia, which, according to Bertin, possesses a magnetic rotatory power less than that of water. The fact is perfectly correct, but it must be interpreted quite otherwise than in the case of the salts of iron. Nitrate of ammonia is so soluble in water that we may easily prepare solutions containing 60 to 66 per cent. of the salt. The magnetic rotation of the plane of polarization produced by these solutions is weaker than that of pure water, but it is much greater than that which would be produced by the quantity of water alone which enters into the solution. The experiment, therefore, merely proves that the nitrate of ammonia gives the solution a rotatory power less than that of water, but still in the same direction.—*Comptes Rendus*, Sept. 8, 1856, p. 529.

ON THE POSSIBILITY OF THE SIMULTANEOUS EXISTENCE OF OPPOSITE ELECTRICAL CURRENTS IN THE SAME CONDUCTING WIRE. BY PROFESSOR G. BELLI.

The author's object in this paper is to show that the galvanometer, by means of which many physicists have endeavoured to solve the above question, is not adapted to furnish its solution. This has already been pointed out by M. Soret, in a particular case, upon the foundation of some considerations exactly analogous to those now put forward in a much more general manner by M. Belli. The latter shows that,—

In any system of conducting wires isolated from the ground, or communicating therewith by one or several points, and submitted to the simultaneous action of several batteries acting with any force and in any direction, the galvanometric effects which must be presented by any one of these wires are precisely the same, whether we regard the currents produced by the differences as independent or not, that is to say, either according to one or the other of the two following theories :—

1. That each battery determines its particular system of currents, and that each wire is traversed simultaneously by all the unaltered partial currents, on condition that the currents of each battery taken separately are subjected to the laws of Ohm, and submitted not only to the resistance of the battery which produces it, but also to that of all the other batteries.

2. That in each wire we have a single current furnished by the composition of the forces with which the different batteries set electricity in motion.

M. Belli has taken up a certain number of particular cases, and shown that the facts constantly agree with the two theories (at least if we admit the conditions enunciated above), and that consequently we must seek for some other method of determining which of the two hypotheses is the true one.—*Il Nuovo Cimento*, vol. ii. p. 401.

## MEASUREMENT OF THE SPEED OF A RAILWAY TRAIN BY MEANS OF ELECTRO-MAGNETISM. BY W. C. M'REA.

The wheels of a car rotate a certain number of times in going over a given space of road ; for example, suppose a car-wheel to be eight feet in circumference, it will rotate 660 times in going the distance of a mile. Now, if the car were so constructed that the body would always remain at an equal distance from the axle, which is prevented by the motion given it by the springs, it would not be difficult to bring the wheel at each revolution to so bear upon a lever, as to rotate a wheel inside the car, which might have as many cogs as the car-wheel rotates times in going a mile. Or, by a series of clockwork wheels, the indicating wheel might contain a fractional number of cogs in proportion to the number of times which the car-wheel would rotate in a given distance.

The difficulty occasioned by the unsteady motion of the car, owing to the springs, may be overcome by the use of a magnet, battery, and galvanic circuit ; the latter to be so arranged as to be broken at each revolution of the car-wheel or axle. So by this means the operation of the magnetic contrivance would be to move the speed-indicator the distance of one cog.

A contrivance of this kind may be so constructed as not to require winding, in order to have it in readiness for use.

The indicator may be so constructed as to have an index placed over it, in such a position that the hand on the indicator should move it a sufficient distance to show at each successive rotation the number of miles already travelled.

The kind of battery most suitable for such a contrivance would be that of the " sand battery," which, if properly constructed, may be

made to act with as much certainty as any other, and at far less expense. This battery should be constructed with sand of such quality as is used by the manufacturers of glass, that is, free from dirt of any kind. The battery cup may be that of the ordinary kind, as used in Grove's battery; or if larger glass or porcelain jars can be obtained, they would doubtless be better, affording an opportunity for the use of larger plates of zinc and copper; these should be placed about an inch apart, and the sand packed tightly around them. The zinc should be amalgamated, and the copper rubbed with a piece of emery-paper before placing in the cup; the sand should then be well moistened with sulphuric acid, diluted in the usual proportion for battery purposes. Intensity may of course be increased by increase of acid. A battery of this kind would stand for some weeks, only requiring the addition of a little acid each day, according as the strength of that first put in became exhausted. A series of cups of this kind could be carried on a train without the difficulties which would arise from that of almost any other battery.—*Journal of the Franklin Institute,* October 1856, p. 217.

## METEOROLOGICAL OBSERVATIONS FOR OCT. 1856.

*Chiswick.*—October 1. Exceedingly fine. 2. Cloudy. 3. Cloudy and fine : rain. 4. Rain : cloudy : rain. 5. Rain : cloudy : fine : lightning at night. 6. Foggy : rain : cloudy. 7. Hazy and drizzly : rain at night. 8. Heavy rain : densely overcast. 9. Overcast. 10. Hazy : fine. 11. Heavy rain : uniform haze : slight rain at night. 12. Foggy : uniform haze. 13. Dense fog : very fine : cloudy. 14. Foggy : overcast : fine. 15. Rain. 16. Overcast : showery. 17. Fine : overcast : hazy. 18. Very fine. 19. Dense fog : very fine. 20. Foggy : very fine. 21. Foggy : exceedingly fine. 22. Very dense fog : exceedingly fine. 23. Foggy : very fine. 24. Fine. 25. Foggy : heavy clouds : fine. 26. Hoar-frost : hazy : very fine. 27. Frosty : haze : fine. 28, 29. Very dense fog. 30. Hazy clouds : overcast : fine. 31. Uniform haze : rain : cloudy.

Mean temperature of the month ............................. 51°·28
Mean temperature of Oct. 1855 ............................. 50 ·28
Mean temperature of Oct. for the last thirty years ............ 50 ·00
Average amount of rain in Oct. ................................. 2·802 inches.

*Boston.*—Oct. 1, 2. Fine. 3. Fine : rain P.M. 4. Rain A.M. and P.M. 5. Cloudy. 6. Fine. 7, 8. Rain A.M. and P.M. 9—12. Cloudy : rain A.M. and P.M. 13. Cloudy. 14. Fine. 15. Fine : rain A.M. and P.M. 16. Cloudy : rain P.M. 17. Fine. 18. Cloudy. 19. Fine. 20, 21. Cloudy. 22, 23. Foggy. 24. Fine. 25—28. Cloudy. 29. Foggy. 30, 31. Cloudy : rain A.M.

*Sandwick Manse, Orkney.*—Oct. 1. Bright, fine A.M. : cloudy, fine P.M. 2. Bright, fine A.M. : drops P.M. 3. Hazy A.M. : drops P.M. 4. Hazy A.M. : drops, showers P.M. 5. Showers A.M. : clear, aurora P.M. 6. Clear, fine, hoar-frost A.M. : clear, aurora P.M. 7. Clear, fine, hoar-frost A.M. : cloudy P.M. 8. Bright A.M. : cloudy P.M. 9. Cloudy, fine A.M. : clear P.M. 10. Cloudy, fine A.M. and P.M. 11. Bright, hazy A.M. : cloudy P.M. 12. Clear, fine A.M. and P.M. 13. Bright, fine A.M. : cloudy P.M. 14. Damp A.M. : clear P.M. 15. Damp A.M. : cloudy P.M. 16. Showers A.M. : drizzle P.M. 17. Cloudy A.M. : rain, clear P.M. 18. Bright A.M. : drizzle P.M. 19. Bright, fine A.M. : drizzle P.M. 20. Bright, fine A.M. : clear, aurora P.M. 21. Fog A.M. : cloudy P.M. 22. Cloudy, fine A.M. : showers P.M. 23. Clear, fine A.M. : clear, aurora P.M. 24. Clear, frost, fine A.M. : clear, aurora P.M. 25. Damp A.M. : rain F.M. 26. Drizzle A.M. : damp P.M. 27. Drizzle A.M. and P.M. 28. Damp A.M. : cloudy P.M. 29. Hazy, fine A.M. : clear P.M. 30. Cloudy, fine A.M. : cloudy P.M. 31. Cloudy, fine A.M. : clear, fine P.M.

Mean temperature of Oct. for previous twenty-nine years ... 47°·57
Mean temperature of this month ................................. 48 ·84
Mean temperature of Oct. 1855 ................................. 45 ·72
Average quantity of rain in Oct. for previous sixteen years ... 5·09 inches.

*Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.*

| Days of Month. 1856. Oct. | Barometer | | | | | Thermometer | | | | | Wind | | | Rain | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Chiswick Max. | Chiswick Min. | Boston 8½ a.m. | Orkney, Sandwick 9¼ a.m. | Sandwick 8¼ p.m. | Chiswick Max. | Chiswick Min. | Boston 8½ a.m. | Orkney, Sandwick 9¼ a.m. | 8¼ p.m. | Chiswick 1 p.m. | Boston | Orkney, Sandwick | Chiswick | Boston | Orkney, Sandwick |
| 1. | 29·818 | 29·796 | 29·40 | 29·67 | 29·68 | 64 | 42 | 45 | 48 | 48 | sw. | sw. | e. | .... | .... | .... |
| 2. | 29·820 | 29·758 | 29·32 | 29·66 | 29·67 | 67 | 50 | 54 | 52 | 52 | s. | sse. | se. | .... | .... | .... |
| 3. | 29·803 | 29·737 | 29·35 | 29·65 | 29·60 | 66 | 55 | 57 | 53½ | 52 | sw. | s. | s. | ·10 | .... | ·02 |
| 4. | 29·735 | 29·637 | 29·11 | 29·54 | 29·55 | 66 | 56 | 60 | 51 | 40½ | sw. | s. | calm | ·11 | ·24 | ·02 |
| 5. | 29·884 | 29·807 | 29·30 | 29·83 | 29·99 | 63 | 42 | 60 | 47½ | 36½ | e. | w. | ne. | ·03 | ·08 | ·02 |
| 6. | 29·944 | 29·922 | 29·52 | 30·16 | 30·25 | 57 | 55 | 55 | 42 | 45 | ne. | ne. | calm | ·06 | .... | .... |
| ☽ 7. | 29·926 | 29·897 | 29·53 | 30·17 | 30·12 | 59 | 52 | 52 | 44½ | 49½ | ne. | ne. | ssw. | ·36 | ·28 | .... |
| 8. | 30·066 | 29·904 | 29·48 | 30·06 | 30·05 | 58 | 49 | 53 | 50 | 48 | ne. | n. | wsw. | ·50 | ·20 | .... |
| 9. | 30·211 | 30·122 | 29·70 | 30·08 | 30·16 | 60 | 49 | 50 | 52 | 49½ | ne. | n. | s. | ·30 | ·08 | .... |
| 10. | 30·199 | 30·106 | 29·75 | 30·22 | 30·22 | 62 | 43 | 55 | 52½ | 49½ | ne. | e. | calm | ·12 | ·68 | .... |
| 11. | 30·039 | 29·972 | 29·62 | 30·16 | 30·09 | 60 | 52 | 56 | 50½ | 47 | s. | ne. | se. | ·02 | ·06 | .... |
| 12. | 30·160 | 30·045 | 29·57 | 30·03 | 30·01 | 64 | 42 | 55 | 49½ | 50 | s. | s. | s. | .... | .... | .... |
| ○ 13. | 30·193 | 30·108 | 29·70 | 30·08 | 30·05 | 67 | 44 | 56 | 52½ | 49½ | se. | se. | se. | ·20 | ·21 | ·09 |
| 14. | 29·945 | 29·694 | 29·52 | 29·93 | 29·76 | 60 | 48 | 57 | 50 | 51½ | s. | s. | se. | ·21 | ·13 | ·21 |
| 15. | 29·654 | 29·389 | 29·04 | 29·57 | 29·53 | 61 | 47 | 53 | 51½ | 49 | nw. | wnw. | sse. | ·06 | ·05 | ·02 |
| 16. | 30·262 | 29·985 | 29·50 | 29·50 | 29·79 | 59 | 43 | 50 | 53 | 46 | sw. | sw. | w. | .... | .... | ·02 |
| 17. | 30·318 | 30·308 | 29·83 | 29·67 | 30·00 | 59 | 46 | 53·5 | 55½ | 48 | sw. | sw. | e. | .... | .... | .... |
| 18. | 30·310 | 30·270 | 29·84 | 30·22 | 30·19 | 63 | 32 | 43·5 | 42½ | 52 | sw. | e. | sse. | .... | .... | .... |
| 19. | 30·286 | 30·237 | 29·78 | 30·11 | 30·16 | 60 | 42 | 55·5 | 52 | 46½ | e. | e. | sw. | .... | .... | ·01 |
| ☾ 20. | 30·181 | 30·143 | 29·75 | 30·22 | 30·15 | 63 | 39 | 52·5 | 45½ | 49½ | e. | calm | w. | .... | .... | ·05 |
| 21. | 30·290 | 30·190 | 29·90 | 30·05 | 30·05 | 64 | 36 | 48·5 | 53 | 50½ | sw. | s. | calm | .... | .... | .... |
| 22. | 30·308 | 30·282 | 29·85 | 30·11 | 30·42 | 68 | 42 | 52 | 48 | 43 | e. | calm | calm | .... | .... | .... |
| 23. | 30·417 | 30·304 | 30·00 | 30·48 | 30·48 | 62 | 50 | 52 | 40 | 40 | ne. | sw. | sw. | ·01 | .... | .... |
| 24. | 30·484 | 30·411 | 30·05 | 30·31 | 30·42 | 58 | 40 | 50 | 50 | 51 | e. | ne. | wsw. | .... | .... | .... |
| 25. | 30·471 | 30·444 | 30·00 | 30·24 | 30·28 | 56 | 26 | 50 | 52½ | 52½ | e. | nw. | w. | .... | .... | ·19 |
| 26. | 30·424 | 30·365 | 30·00 | 30·17 | 30·23 | 52 | 25 | 41 | 53 | 51½ | e. | calm | se. | .... | .... | ·01 |
| 27. | 30·345 | 30·321 | 29·90 | 30·26 | 30·26 | 55 | 26 | 41 | 51 | 49½ | e. | nw. | se. | ·01 | .... | ·05 |
| ● 28. | 30·339 | 30·268 | 29·84 | 30·06 | 30·16 | 51 | 37 | 42·5 | 49½ | 43 | ne. | se. | se. | ·01 | .... | .... |
| 29. | 30·230 | 30·184 | 29·74 | 29·77 | 29·96 | 45 | 35 | 50 | 50 | 49½ | s. | se. | s. | ·02 | .... | ·19 |
| 30. | 30·156 | 30·130 | 29·66 | 29·91 | 29·75 | 58 | 46 | | 52 | 46 | s. | ssw. | sw. | ·28 | .... | ·01 |
| 31. | 30·313 | 30·097 | | | 30·29 | 58 | | | | | | sw. | | | ·07 | ·15 |
| Mean. | 30·149 | 30·059 | 29·66 | 30·010 | 30·030 | 60·16 | 42·41 | 51·6 | 49·76 | 47·93 | | | | 2·40 | 2·08 | 1·01 |

LXV. *Memoir on the Specific Heat of some Simple Bodies, and on the Isomeric Modifications of Selenium. By* M. V. REG-NAULT[*].

**D**ULONG and Petit were the first to announce the remark-able physical law, that " the specific heats of simple bodies are reciprocally proportional to their atomic weights." The experiments on which they had established this law were not numerous, and many of them have been found to be contradictory, in consequence of the considerable changes which more exact analyses, and a more complete knowledge of chemical compounds, have produced in the adopted atomic weights of some simple bodies. Nevertheless the numerous experiments which I made fifteen years ago, on all the simple bodies which I could procure in sufficient quantity, proved that the law of Dulong and Petit ought to be admitted; not, indeed, in the rigorous acceptation which those celebrated physicists had claimed for it, but as an approximative law, which may be usefully appealed to in many scientific considerations. In fact, according to the precise enun-ciation of this law, the product of the specific heat of a simple body by its atomic weight ought to be a constant number; whereas in my experiments on solid bodies, this product varied from 36 to 41. This variation arises from the fact, that the calorific capacity of bodies, as determined in our experiments, comprehends not only the *specific atomic heat*, that is to say, the quantity of heat necessary to elevate the temperature of the atom one degree, but also the heat which disappears in causing the expansion of the body, or in the molecular changes which pro-duce its successive softening, or determine the variations of its crystalline groups. These secondary effects absorb quantities of caloric which are by no means reciprocal to the atomic weights of the bodies, and each of which varies according to the limits of the temperature within which it is observed.

[*] From the *Annales de Chimie et de Physique* for March 1856.

To be convinced of this, it is sufficient to compare between themselves the specific heats of some simple bodies taken at different points of the thermometric scale; it is immediately seen that their relations change sensibly with the temperature ; and as the atomic weights remain the same, it is evident that the product of the specific heat by the atomic weight may vary with the temperature unequally in each case.

In short, the specific heat of the same body is often very different, according as it is estimated in the solid, liquid, or gaseous state.    Thus, while for all the solid simple bodies which I studied in my previous memoirs the product of the specific heat by the atomic weight varied between 36 and 41, the same product is only 24 for those simple gases, such as oxygen, hydrogen, and nitrogen, which deviate little from Mariotte's law, and it is 29·5 for chlorine and gaseous bromine, which show a considerable deviation.    Further, the gases which follow Mariotte's law pretty closely, maintain their calorific capacity sensibly constant at different points of the thermometric scale ; while this calorific capacity increases rapidly with the temperature in those gases which deviate considerably from this law, as is the case with carbonic acid.    This last observation would of itself be sufficient to prove that the law of Dulong and Petit cannot be admitted as an exact physical law.

However this may be, no one doubts now that the law of Dulong and Petit, when only applied to simple bodies in the same physical condition, can be of great help in chemistry as a guide in selecting atomic weights for simple bodies, when chemical considerations permit several equally probable to be adopted. It was from reasoning on this law that I proposed to change the atomic weights of the alkaline metals, and of that of silver, and to take values half those which chemists have usually adopted. It cannot be long before this change, which is in accordance with isomorphism, is adopted.

Convinced of the utility of obtaining, as speedily as possible, the specific heats of simple bodies which have not yet been determined, I have never neglected to make experiments when I was able to procure any of these bodies in a sufficient quantity and in a sufficiently pure condition. The Universal Exhibition which took place this year at Paris afforded me a favourable opportunity, by putting at my disposal simple bodies, often very pure, and on which I had not hitherto made experiments.

These new experiments were made with the apparatus and by the process described in my first memoir*.    The only modification of the former method consisted in the estimation of the correction which must be made in the elevation of temperature

* *Annales de Chimie et de Physique,* 2nd series, vol. lxxviii. p. 20.

undergone by the calorimeter, on account of perturbations produced by external causes. This modification exercises, however, little influence on the results.

The method of proceeding was as follows:—

1. The variation of temperature $\Delta\theta$, which the calorimeter underwent from disturbing causes alone, was observed during three minutes, the water being continually agitated by means of a small stirrer which formed part of the apparatus.

2. The calorimeter was placed rapidly under the oven, in which the little basket containing the substance experimented upon had been raised to a temperature accurately determined, and about 100 degrees. The basket was then quickly lowered into the water of the calorimeter, and this was then returned to its original position in front of the telescope which served for reading the thermometer. It took half a minute to place the calorimeter beneath the oven, and to make the necessary arrangements for lowering the basket; during this time the calorimeter underwent a variation of temperature similar to that which had occurred in the same time during the first period. The lowering of the basket only required a few seconds; and as this basket was immediately agitated, the water assumed in less than a minute its maximum temperature when the substance was a good conductor of heat. At the end of the fourth minute the calorimeter was brought to its first position, and its temperature might be read off.

3. From minute to minute the temperatures of the calorimeter and of the surrounding medium were observed till the seventh minute.

4. Lastly, an observation was made of the variation of temperature which the calorimeter underwent from disturbing causes alone during the three succeeding minutes, that is, from the seventh to the tenth.

Representing by—

A, the value in water of the calorimeter with its appendages, that is, the immersed part of the thermometer and the little agitator;

M, the weight of the substance submitted to experiment;

$p$, the value in water of the basket;

T, the temperature indicated by the thermometer of the oven, corrected for that portion of the column which is outside;

$\theta'$, the temperature of the calorimeter at the end of the seventh minute;

$\Delta\theta$, the elevation of temperature observed between the end of the third minute and that of the seventh;

$\Delta\theta'$, the elevation which it would have undergone if it had not been influenced by disturbing causes;

*c,* the unknown specific heat of the substance ;
  we have

$$(MC + p)(T - \theta') = A\Delta\theta'.$$

Let us suppose that during the first period of three minutes
the temperature of the calorimeter is inferior to that of the sur-
rounding air; it will undergo an increase of heat *a.* If, on the
contrary, this temperature is, during the last period of three
minutes, higher than that of the surrounding air, the calorimeter
would be cooled by a quantity *a'.* It was admitted that for from
3 minutes to $3\frac{1}{2}$ minutes the calorimeter continued to undergo
the same variation of temperature as in the three preceding

minutes, and that it would undergo an elevation of temperature $\dfrac{a}{6}$.

At $3\frac{1}{2}$ minutes the basket was immersed in the calorimeter ; it
was supposed that from $3\frac{1}{2}$ to 4 minutes the temperature of the
calorimeter was not influenced by disturbing causes, because
during this period the calorimeter traversed almost completely
its variation of temperature. From 4 minutes to 7 minutes the
temperature did not change sensibly, and it might be admitted
that the perturbation produced by external causes was equal to
that exhibited during the three succeeding minutes, that is from
7 to 10 minutes.
  According to that we should have

$$\Delta\theta' = \Delta\theta - \frac{a}{6} + a'.$$

This mode of proceeding is only suited for bodies which are
good conductors of heat; for others, a tolerably long time is
necessary before the thermometer of the calorimeter attains its
stationary temperature. It is hence necessary to calculate the
change of temperature which external causes produce during
each minute. If *t* and *t'* represent these surrounding tempera-
tures during the initial and terminal periods of three minutes,
we might put

$$\frac{a}{3} = A(\theta - t) + K$$

$$\frac{a'}{3} = A(\theta' - t') + K,$$

A and K being the constants which would be determined by the
experiments themselves. The variation of temperature produced
in each minute for a temperature *t* of the calorimeter, and an
external temperature *t,* would be

$$A(\theta - t) + K.$$

## Osmium.

I owe to the kindness of M. Fremy the osmium which has served for my experiments; the metal was in the form of spongy masses loosely aggregated, and of a bluish metallic lustre. The following are the results of three experiments :—

|  | I. | II. | III. |
|---|---|---|---|
| M . . . | 60$^{gr}$·63 | 55$^{gr}$·60 | 55$^{gr}$·37 |
| $p$ . . . | 2$^{gr}$·258 | 2$^{gr}$·258 | 2$^{gr}$·258 |
| T . . . | 97°·50 | 97°·49 | 98°·08 |
| $\theta'$ . . . | 8°·50 | 6°·58 | 9°·00 |
| $\Delta\theta'$ . . . | 0°·76485 | 0°·75212 | 0°·72311 |
| A . . . | 483$^{gr}$·02 | 483$^{gr}$·02 | 483$^{gr}$·02 |
| $c$ . . . | 0·0310 | 0·0308 | 0·0301 |

Mean . . . 0·03063.

The equivalent of osmium, according to Berzelius, is 1244·2, which would give to osmic acid the formula $OsO^5$. The product of the equivalent by the specific heat we have found to be 38·109; it is therefore comprised within the two limits 36 and 41 which we have met with in other simple bodies. Hence the value found for the specific heat of osmium verifies the formula which chemists usually assign to the compounds of this metal.

## Rhodium.

The rhodium on which I experimented formed part of the beautiful display of MM. Desmoutis, Chapuis and Co., manufacturers of platinum; it formed a simple forged cylinder, resembling in appearance metallic platinum. In the experiment of determining its specific heat, the cylinder was attached simply by a very fine steel wire, the calorific capacity of which might be neglected.

| M . . . | 98$^{gr}$·07 | 98$^{gr}$·07 | 98$^{gr}$·07 |
|---|---|---|---|
| $p$ . . . | ... | ... | ... |
| T . . . | 97°·52 | 97°·45 | 97°·40 |
| $\theta'$ . . . | 23°·34 | 19°·20 | 23°·25 |
| $\Delta\theta'$ . . . | 0°·9348 | 0°·9749 | 0°·9681 |
| A . . . | 422$^{gr}$·54 | 422$^{gr}$·54 | 422$^{gr}$·54 |
| $c$ . . . | 0·05357 | 0·05368 | 0·05499 |

Mean . . . 0·05408.

According to Berzelius, the equivalent of rhodium is 652·1; the product of the equivalent by the specific heat is thence 35·26; this number is a little less than those which have been found for other simple bodies; but the difference is very small, and

might arise from the presence of a small quantity of foreign metal, probably of iridium.

### Iridium.

I have already given, in my first memoir on the specific heats of simple bodies, some experiments on a specimen of iridium which had been lent to me by M. de Meyendorff. I had found for the specific heat 0·03683, which, multiplied by the equivalent generally adopted, 1233·2, gave 45·43. This number being far too high, I supposed that the metal was impure.

The new specimen on which I experimented came from the Exhibition of MM. Desmoutis, Chapuis and Co.; it formed half of a disc strongly aggregated by percussion, and weighed 509$^{gr}$·21. In the experiment the metal was attached by a fine steel wire.

| M | . . 509$^{gr}$·21 | 509$^{gr}$·21 | 509$^{gr}$·21 | 509$^{gr}$·21 |
|---|---|---|---|---|
| $p$ | . . ... | ... | ... | ... |
| T | . . 97°·87 | 97°·15 | 97°·81 | 97°·47 |
| $\theta'$ | . . 22°·34 | 21°·64 | 24°·15 | 24°·59 |
| $\Delta\theta'$ | . . 3°·3808 | 3°·1787 | 3°·2494 | 3°·1620 |
| A | . . 422$^{gr}$·54 | 422$^{gr}$·54 | 422$^{gr}$·54 | 422$^{gr}$·54 |
| $c$ | . . 0·03703 | 0·03570 | 0·03649 | 0·03597 |

Mean . . . 0·0363.

This value differs very little from that which I found for the iridium of M. de Meyendorff. The product of the specific heat by the atomic weight is 44·76, which is not comprised within the limits of variation which we have observed in other simple bodies. This anomaly may be explained in two ways: either the metal is impure, or its atomic weight is inexact. If the metal is not pure, which appears probable from the low density which I found, it could only be mixed with palladium, rhodium, or ruthenium; for platinum and osmium would not change its specific heat, since they have the same atomic weight. It is scarcely probable that it is palladium, in consequence of the chemical properties of the latter metal, and of the nature of the mineral used in the preparation of iridium. It could only be rhodium or ruthenium, whose atomic weights are only half that of platinum; or some metal still unknown which would be similarly circumstanced. It is desirable that those chemists who are at present engaged on the study of those metals which accompany platinum, would be good enough to direct their attention to this point.

### Aluminium.

The aluminium which I employed in my experiments was prepared by M. Deville; it had been cast in the form of four ingots.

|         | I.          | II.         | III.        | IV.         | V.          | VI.         |
|---------|-------------|-------------|-------------|-------------|-------------|-------------|
| M ...   | $147^{gr}{\cdot}80$ | $147^{gr}{\cdot}80$ | $147^{gr}{\cdot}80$ | $147^{gr}{\cdot}80$ | $147^{gr}{\cdot}80$ | $147^{gr}{\cdot}80$ |
| $p$ ... | $2^{gr}{\cdot}335$  | $2^{gr}{\cdot}335$  | $2^{gr}{\cdot}335$  | $2^{gr}{\cdot}335$  | $2^{gr}{\cdot}335$  | $2^{gr}{\cdot}335$  |
| T ...   | $97°{\cdot}54$  | $97°{\cdot}33$  | $97°{\cdot}21$  | $96°{\cdot}82$  | $97°{\cdot}59$  | $97°{\cdot}76$  |
| $\theta'$ ... | $25°{\cdot}81$ | $28°{\cdot}07$ | $24°{\cdot}02$ | $26°{\cdot}18$ | $23°{\cdot}92$ | $22°{\cdot}68$ |
| $\Delta\theta'$ ... | $5°{\cdot}5197$ | $5°{\cdot}3866$ | $5°{\cdot}6361$ | $5°{\cdot}4628$ | $5°{\cdot}6974$ | $5°{\cdot}7821$ |
| A ...   | $422^{gr}{\cdot}54$ | $422^{gr}{\cdot}54$ | $422^{gr}{\cdot}54$ | $422^{gr}{\cdot}54$ | $422^{gr}{\cdot}54$ | $422^{gr}{\cdot}54$ |
| $c$ ... | $0{\cdot}20471$ | $0{\cdot}20809$ | $0{\cdot}20436$ | $0{\cdot}20581$ | $0{\cdot}20581$ | $0{\cdot}20490$ |

Mean............ $0{\cdot}20556$.

The product of this specific heat by the equivalent 170·98, which is usually given to aluminium, is 35·15. This number is a little less than the analogous products obtained for other simple bodies; but this seems to indicate that the metal on which I operated was far from being pure. In fact, according to an analysis which M. Salvétat was good enough to make at my request, the metal contained,—

| | |
|---|---|
| Silicium | 2·87 |
| Iron | 2·40 |
| Copper | 6·38 |
| Lead | traces |
| Aluminium (by difference) | 88·35 |
| | 100·00 |

We can calculate very closely the specific heat of pure aluminium from that which we have found for the impure metal, and from the specific heats of the metals which are mixed with it. It is merely necessary to make a supposition on the specific heat of silicium which has never yet been determined. Let us assume that it is 0·1425, because the product of this specific heat by the equivalent 266·7 of silicium is 38. But this presupposes that the formula of silicic acid is $SiO^3$, which is far from being demonstrated. Whatever it may be, the error which would result from the falsity of this assumption would be of little importance for the object which at present occupies us. We should find thus for the specific heat of pure aluminium 0·2181; and the product of this number by the equivalent of aluminium is 37·29, which is entirely within the prescribed limits.

Hence the value which we have found for the specific heat of pure aluminium confirms the formula $Al^2O^3$ which chemists have been led to adopt for alumina, from the isomorphism of this substance with the sesquioxide of iron.

Since then I have succeeded in procuring purer aluminium which had been prepared by M. Rousseau; it only contained traces of silicon, and 2 per cent. of metallic iron. Two deter-

minations which I made of the specific heat have given the following results :—

| M | . . . . | 134$^{gr}$·81 | 124$^{gr}$·595 |
| p | . . . . | 2$^{gr}$·030 | 2$^{gr}$·030 |
| T | . . . . | 97°·44 | 97°·31 |
| $\theta'$ | . . . . | 15°·51 | 13°·71 |
| $\Delta\theta'$ | . . . . | 5°·9445 | 5°·6308 |
| A | . . . . | 422$^{gr}$·54 | 422$^{gr}$·54 |
| c | . . . . | 0·21236 | 0·21212 |

Mean . . . 0·21224.

If the 2 per cent. of metallic iron which the metal contains be taken into account, the specific heat of pure aluminium will be found to be 0·2143, and the product of this specific heat by the equivalent is 36·64.

### Cobalt.

The display of Messrs. Johnson and Matthey of London contained some beautiful samples of cobalt and nickel; I have taken advantage of them to make some new determinations of their specific heat. Unfortunately both the metals contained carbon, particularly the nickel.

| M | . . . . | 218$^{gr}$·29 | 218$^{gr}$·29 |
| T | . . . . | 97°·84 | 97°·83 |
| $\theta'$ | . . . . | 25°·90 | 21°·80 |
| $\Delta\theta'$ | . . . . | 4°·3861 | 4°·6110 |
| A | . . . . | 422$^{gr}$·54 | 422$^{gr}$·54 |
| c | . . . . | 0·10686 | 0·10706 |

Mean . . . 0·10696.

The product of this specific heat by the atomic weight 369 gives 39·47. This specific heat is probably a little too great; it is however identical with that which I found for cobalt in my former experiments.

### Nickel.

In my previous experiments, I found for nickel prepared by the calcination of the oxalate in a closed vessel the specific heat 0·1086. My new experiments have given a slightly different value; but the nickel of Mr. Johnson was very carburetted, as was easy to observe on dissolving it in hydrochloric acid.

| M | . . . . | 229$^{gr}$·78 | 229$^{gr}$·78 |
| T | . . . . | 97°·92 | 97°·93 |
| $\theta'$ | . . . . | 21°·41 | 20°·46 |
| $\Delta\theta'$ | . . . . | 5°·0295 | 5°·0854 |
| A | . . . . | 452$^{gr}$·54 | 422$^{gr}$·54 |
| c | . . . . | 0·1110 | 0·1109 |

Mean . . . 0·11095.

The product of this specific heat by the atomic weight 369·7, usually given to nickel, is 41·00. It is probable that the specific heat of pure nickel is considerably less, and that it does not exceed 0·103.

## Sodium.

My former researches on the calorific capacity of the binary compounds, and of the salts formed by the alkaline metals, showed that it was necessary to divide by 2 the atomic weights which have hitherto been adopted by chemists*. Direct researches on metallic potassium have led me since then to the same conclusion†. It was of importance to verify this result with metallic sodium, which is now procured so easily in a great state of purity, and the manipulation of which does not present so many difficulties as does that of potassium. M. Liebig sent me, about six years ago, a certain quantity of sodium which he had had expressly prepared for this purpose in his laboratory at Giessen ; but my various occupations have until now prevented me from making the experiment. I made determinations on the sodium of M. Liebig which had been perfectly well kept in hermetically sealed tubes, and on sodium recently prepared by M. Rousseau.

On breaking the tubes which contained the sodium of M. Liebig, I was able to confirm the fact that the metal presented two very distinct physical conditions. In the one, the sodium was perfectly malleable, could be bent in every direction without breaking, and presented no trace of crystallization. In the other, on the contrary, the metal was extremely brittle, and exhibited in its fracture pentagonal dodecahedra of the most perfect regularity. These dodecahedra could be even detached from the mass with the greatest ease, and presented then most perfect crystals, whose diameter sometimes exceeded 3 millimetres. This difference was owing probably to the presence of a small quantity of potassium.

I cast the melted sodium under naphtha in a brass mould, fig. 1. This mould is composed of a cylinder, *abcd*, slightly conical, on which is screwed a bottom, *cd*, pierced by a central hole, *f*. In this hole is fitted a cylinder, also slightly conical. The melted substance is run into the circular space ; when it has solidified on cooling, the bottom, *cd*, is unscrewed, the substance is taken out as well as the cylinder *ef*, and the sodium is obtained in the form of a cylinder bored in the direction of its axis. The cylinder of sodium was wiped dry immediately on cooling ; the external surface

Fig. 1.
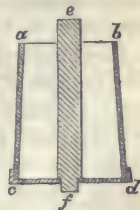
which had been in contact with the air was cut off and the cylinder immediately covered with tinfoil, the weight of which had been previously determined. It could then be weighed at leisure, and the experiment for determining its calorific capacity be arranged. I have thought it necessary to determine it, at temperatures below 0 degree, in order to avoid the anomalies proceeding from the successive softening which sodium suffers at more elevated temperatures.

In order to subject the substance to a uniform temperature, and susceptible of an accurate determination, I used the apparatus represented at fig. 2, which may serve also for higher

Fig. 2.
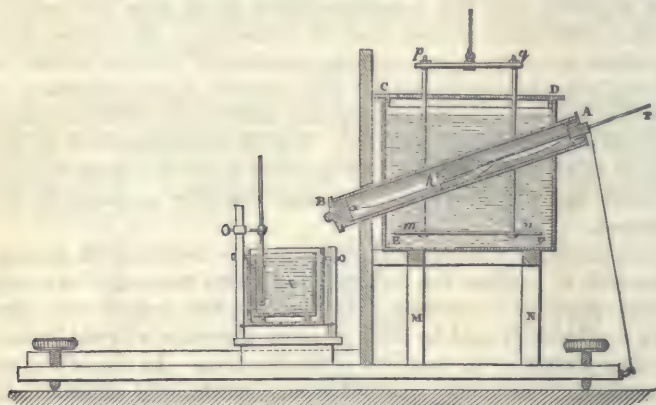


temperatures. This apparatus is composed of an oblong trough, C D E F, resting on a support M N. An inclined tube of brass traverses this trough, and its two open extremities project at the exterior. It is in the middle of this tube that the substance is kept which has to be raised to a fixed temperature. In the present case, the cylinder of sodium, K, was fixed by silk threads which traversed the two orifices of the tube AB, and were maintained by the two caps which closed them. A thermometer, T, the bulb of which is inserted in the central hole of the sodium cylinder, is fixed by means of a stopper on the cap A. The trough is filled with a mixture of pounded ice and crystallized chloride of calcium, which is constantly agitated. The temperature gradually descends, but some time is necessary before the minimum is reached, which then rests stationary for ten or fifteen minutes. It is at this time that the calorimetric experiment is made.

In the calorimeter, V, a determinate quantity of naphtha was placed, which had been previously distilled over metallic

sodium. But it was necessary to determine the value in water of the calorimeter thus arranged. With this view, I made on the calorimeter the determination of the calorific capacity of lead between the surrounding temperature and that of boiling water. As the specific heat of lead is known within these limits of temperature, it was easy to deduce from the experiment the calorific value in water of the calorimeter filled with naphtha. The following are the results of the experiment:—

| | | |
|---|---|---|
| Weight of lead . . . | 636$^{gr}$·45 | 636$^{gr}$·45 |
| Specific heat of lead . . | 0$^{gr}$·03140 | 0$^{gr}$·03140 |
| T . . . . . . . | 96°·77 | 97°·076 |
| $\theta'$ . . . . . . . | 14°·165 | 15°·085 |
| $\Delta\theta'$ . . . . . . . | 10°·1691 | 10°·1691 |
| Log A . . . . . . | 2·2071712 | 2·2104166 |

Mean of log A = 2·2087939.

I wished to verify this value by an experiment made on the same mass of lead cooled in the refrigerating mixture: I obtained hence—

| | |
|---|---|
| P . . . . . | 636$^{gr}$·45 |
| T . . . . . | −28°·030 |
| $\theta'$ . . . . . | + 6°·31 |
| $\Delta\theta'$ . . . . . | 4°·2795 |
| Log A . . . . | 2·1994784 |

I have assumed that the specific heat of lead between −28 degrees and the surrounding temperatures was 0·0310. I found, in fact,—

| | | | | |
|---|---|---|---|---|
| Between +15° and | 98° | 0·03140 | . . . . . | (1) |
| ... −78° ... | +10° | 0·03065 | . . . . . | (2) |

and the mean 0·310 may be admitted as applying to the temperatures comprised between −28 and +7 degrees.

The value of log A given by this experiment is a little less than that furnished by the two preceding. I have adopted the mean of the three values, that is 2·2056887.

Here are the results which I obtained with sodium:—

| | Sodium of M. Liebig. | | Sodium of M. Rousseau. | |
|---|---|---|---|---|
| | I. | II. | III. | IV. |
| Weight of sodium | 58$^{gr}$·918 | 58$^{gr}$·918 | 56$^{gr}$·580 | 56$^{gr}$·580 |
| Weight of tinfoil | 2$^{gr}$·152 | 2$^{gr}$·152 | 2$^{gr}$·700 | 2$^{gr}$·700 |
| T . . . . . | −34°·573 | −30°·126 | −29°·276 | −21°·628 |
| $\theta'$ . . . . . | 4°·7121 | 5°·0756 | 7°·242 | 7°·041 |
| $\Delta\theta'$ . . . . | 4°·2787 | 3°·9356 | 3°·7913 | 3°·0315 |
| $c$ . . . . . . | 0·29480 | 0·30267 | 0·29205 | 0·29742 |

Specific heat of tin . . 0·0562.

The experiments Nos. II. and IV. were made with cylinders

of sodium which had been used for the experiments Nos. I. and
II. To this end, immediately on taking the sodium out of the
naphtha, the tinfoil was unfolded and rapidly wiped dry with
tissue-paper, as also was the metal itself, and the cylinder imme-
diately replaced in the tube surrounded by the cooling mixture.
The experiments Nos. III. and IV. are less precise than Nos. I.
and II.; first, because the metal had undergone a more profound
alteration in contact with the air, and then because the tempera-
ture of the cooling mixture having begun to ascend, the thermo-
meter at the moment of the experiment only marked a relative
minimum, which did not continue, and which applied less exactly
to the whole mass of the metal than the absolute and stationary
minimum at which the determinations Nos. I. and III. had been
made. It is hence better to take only the mean between Nos. I.
and III., which would give for the specific heat of metallic sodium
0·2934.

The product of this specific heat by the atomic weight 287·2,
which chemists assign to this metal, is 84·2; but the product
by 143·6, the half of this atomic weight, is 42·1. This latter
product alone is comprised within the limits which we have recog-
nized for other simple bodies. It is therefore evident that the
atomic weight of sodium is 143·6, and that the formula of soda
ought to be written $Na^2O$, and not $NaO$.

It is assumed that the fusing-point of sodium is about 90°.
I wished to determine it with accuracy. I poured melted sodium
into a large glass tube, dipped in the melted metal a mercurial
thermometer, and observed the course of its cooling. In many
experiments the thermometer became stationary at a temperature
which, corrected for that portion of the column which was not
plunged in the metal, corresponded precisely to 97°·63. The
stationary temperature observed during the cooling is therefore
97°·63, and it is the only element of this kind susceptible of an
accurate determination.

### Potassium.

I have not thought it necessary to make new determinations
of the specific heat of potassium; the experiments already on
record (*Ann. de Chim. et de Phys.* 3rd series, vol. xxvi. p. 266)
are sufficient for the conclusions which may be drawn as to the
atomic weight of this metal. As potassium undergoes a gradual
softening with the temperature, its specific heat experiences a
material increase, and its value may be found perceptibly too
great. But I have made some experiments for determining
exactly its fusing-point, which treatises of chemistry generally
place at about 55 degrees.

Potassium prepared by M. Rousseau was melted in a large
glass tube under naphtha. After the solidification of the metal,

the whole of the naphtha was removed, and the metal having been again brought to the liquid state, the thermometer was plunged into it, and the heat raised to above 100 degrees. The progress of the cooling of the thermometer was observed, the tube being kept in the surrounding air. In a former experiment the thermometer showed no rest at any point; it descended gradually from 100 to 20 degrees. But in constructing a delineation of the curve of cooling, it was easy to observe that the curve completely changed its form about 55 degrees; its ordinates underwent from this point a much less rapid diminution. I recommenced the experiment, and agitated the thermometer slightly with a view of overcoming the molecular inertia, and of ascertaining the diminution of fluidity. The thermometer descended regularly as far as the division 325 of its scale, then it rose suddenly to 327·2, at which point it was steady for some seconds; it then resumed its downward march, although more slowly than before. The fluidity of the metal diminished gradually; it had a buttery consistence towards 300 divisions; but it was only at 280 divisions that it was no longer possible to move the thermometer.

The 327·2 divisions of the thermometer, corrected for that portion of the column not immersed, correspond to 55°·43. It might be concluded from thence, that the temperature at which the tendency to crystallization commences to increase in liquid potassium is 55°·43, but that it acts with far too little energy to resist the cooling, and bring about a stationary temperature, as is the case with sodium, and generally with bodies which solidify suddenly. The following are the series of temperatures which I have observed : by taking the time as absciss, and the temperature as ordinate, the curve may be graphically represented, and we can observe the influence which the heat disengaged gradually by the solidification has had on the progress of the cooling.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 0' | 75·42 | 4·17 | | 6½ | 50·60 | 0·86 |
| 0½ | 71·25 | 3·77 | | 7 | 49·60 | 1·00 |
| 1 | 67·48 | 3·50 | | 7½ | 48·62 | 0·98 |
| 1½ | 63·98 | 3·37 | | 8 | 47·53 | 0·99 |
| 2 | 60·61 | 2·91 | | 8½ | 46·45 | 1·08 |
| 2½ | 57·70 | 2·46 | | 9 | 45·25 | 1·20 |
| 3 | 55·24 | ... | | 9½ | 44·00 | 1·25 |
| 3¼ | 55·43 | 0·33 | | 10 | 42·80 | 1·20 |
| 3½ | 55·10 | 0·73 | | 10½ | 41·52 | 1·28 |
| 4 | 54·37 | 0·63 | | 11 | 40·28 | 1·24 |
| 4½ | 53·74 | 0·75 | | 11½ | 39·00 | 1·28 |
| 5 | 52·99 | 0·74 | | 12 | 37·72 | 1·28 |
| 5¼ | 52·25 | 0·79 | | 12½ | 36·50 | 1·22 |
| 6 | 51·46 | | | 13 | 35·32 | 1·18 |

It will be seen from this Table that the rapidity diminished regularly up to the third minute, as it ought to do according to the law of Newton, for the quantity of heat lost is proportional to the excess of the temperature of the body above the surrounding medium.    From the third minute, the heat disengaged by solidification commenced to disturb the law of cooling; a remarkable point is observed on the curve, which is disturbed in its former course up to the thirteenth minute at which the observation is discontinued.    Hence when the potassium had attained the temperature of 35 degrees in its cooling after liquefaction, it had even then not entirely disengaged its latent heat of fusion*.

### *Lithium.*

It would be most interesting to know the specific heat of lithium, for it could be decided thence whether lithium was to be ranged among the alkaline metals with sodium and potassium, or among the metals of the alkaline earths with barium and calcium; in other words, whether the formula of lithia ought to be written $Li^2O$ or $LiO$.    Bunsen has lately given a very convenient method of preparing lithium by electrolysis.    I tried this process, and it succeeded admirably; but it would have been difficult to prepare with the chloride of lithium which I was able to procure, a quantity of metal necessary for an exact determination of its calorific capacity.    I preferred to operate at first on chloride of lithium, with the intention of using afterwards the metal itself if I could succeed in procuring it in sufficient quantity.

The chloride of lithium was prepared from carbonate of lithia, from the Exposition of M. Batka of Prague.    The carbonate was first washed with cold water, then dissolved in hydrochloric acid, the solution evaporated to dryness, and then treated when cold with strong alcohol.    The substance dissolved almost entirely. The filtered liquid was again evaporated, and the chloride of lithium melted in a silver crucible.    The liquid matter was poured into a small brass mould, from which it was extracted on cooling in the form of a cylinder pierced in the direction of its axis.    As chloride of lithium is at least as deliquescent as chloride of calcium, I folded it immediately in tinfoil and suspended it in the oven for specific heats.    The calorimetric experiment

* I determined, by the same method, the stationary temperatures which occur during the crystallization of sulphur and of iodine after their fusion. In these two bodies the thermometer rests suddenly, and remains stationary for several minutes.

I found—

For the temperature of the crystallization of sulphur...   113°·60
For the temperature of the crystallization of iodine  ...   113°·58

These two temperatures are obviously identical.

was made by placing it in naphtha in the calorimeter. The results are as follow :—

| | | |
|---|---|---|
| Chloride of lithium . | $62^{gr}\cdot030$ | $82^{gr}\cdot100$ |
| Tin . . . . . . . | $2^{gr}\cdot155$ | $2^{gr}\cdot365$ |
| T . . . . . . . | $96°\cdot69$ | $97°\cdot01$ |
| $\theta'$ . . . . . . . | $12°\cdot88$ | $13°\cdot79$ |
| $\Delta\theta'$ . . . . . . | $8°\cdot9338$ | $11°\cdot6339$ |

I did not endeavour to determine directly the specific heat of chloride of lithium from the quantity of heat acquired by the calorimeter charged with naphtha. 1 preferred to compare it directly with that of chloride of sodium placed under exactly the same conditions. I cast therefore melted chloride of sodium in the same mould, folded it in tinfoil, and determined its calorific capacity with the calorimeter charged with the same quantity of naphtha. The following is the table of results :—

| | | |
|---|---|---|
| Chloride of sodium . | $79^{gr}\cdot410$ | $69^{gr}\cdot330$ |
| Tin . . . . . . . | $1^{gr}\cdot895$ | $1^{gr}\cdot525$ |
| T . . . . . . . | $98°\cdot09$ | $97°\cdot31$ |
| $\theta'$ . . . . . . . | $13°\cdot28$ | $15°\cdot85$ |
| $\Delta\theta'$ . . . . . . | $8°\cdot7369$ | $7°\cdot3532$ |

The specific heat of chloride of sodium was found to be $0\cdot21401$ in my former experiments (*Ann. de Chim. et de Phys.* 3rd series, vol. i. p. 154). By introducing this value into the two determinations, the elements of which I have just given, we can calculate the value in water of the calorimeter charged with naphtha; we find thus,—

| | | | |
|---|---|---|---|
| According to experiment No. I. | . . | . | $166\cdot03$ |
| ...          ... | II. | . . | $166\cdot44$ |
| | Mean . | . . . | $166\cdot24$ |

The two experiments on chloride of lithium, calculated with this element, give for the specific heat of this body,—

| | | | |
|---|---|---|---|
| According to experiment No. I. | . . | . | $0\cdot28096$ |
| ...          ... | II. | . . | $0\cdot28331$ |
| | Mean . | . . . | $0\cdot28213$ |

The product of this specific heat by the atomic weight $524\cdot90$, usually attributed to chloride of lithium, is $148\cdot09$. Now we have found for this product, with the chlorides $R^2 Cl^2$,—

| | | |
|---|---|---|
| Chloride of potassium | . . . . | $161\cdot19$ |
| Chloride of sodium . | . . . . | $156\cdot97$ |

The number obtained for chloride of lithium is obviously less; but the difference is in that sense in which it might be presupposed; for we may remark in general, that *for bodies of the same*

chemical composition, the product of the specific heat by the atomic weight is the lower the less the atomic weight is.

Besides, for the chlorides of the formula R Cl², this product is—

For chloride of calcium . . . 114·72
For chloride of barium . . . 116·44

I think it may be thence concluded, that the formula of chloride of lithium is $R^2 Cl^2$; and that lithium ought to be ranged among the alkaline metals, and not among the alkaline earths. Nevertheless, as the product of the specific heat by the atomic weight is decidedly less for chloride of lithium than for the chlorides of potassium and sodium, it would be advantageous to confirm this conclusion by a determination of the specific heat of the isolated metal; this I shall not fail to do if I succeed in procuring a sufficient quantity of it.

### Tellurium.

I made formerly some determinations of the specific heat of tellurium, by uniting the various specimens which were to be found in the collections, and in the possession of the dealers in chemical products in Paris. I found thus the number 0·05155, which, multiplied by 806·5, the equivalent of the body, gives the number 41·57. This product is a little more than that generally found for simple bodies, the purity of which is not open to suspicion.

The Austrian display presented beautiful specimens of tellurium, which had been prepared at the Mint of Vienna by M. Alexander Löwe. I did not allow the opportunity to escape of making new experiments on a product which I had reason to believe was purer than that with which my previous experiments had been made. A notice published by M. Löwe indicates the mode of preparation which he had followed. Here is a brief abstract of it :—

The mineral is the auriplumbiferous tellurium of Offenbanya in Transylvania, which contains on an average in 100 parts,— 3 parts of gold, 5 of tellurium, 30 of lead in the state of sulphuret, and a quartzose gangue. This mineral, reduced to the state of coarse powder, was treated with weak hydrochloric acid to dissolve the carbonates, then attacked in a leaden vessel by concentrated sulphuric acid, which converted the galena into insoluble sulphate of lead, and dissolved the tellurium as sulphate of oxide of tellurium. The liquid, clarified by having been allowed to stand, was transferred into another leaden vessel containing water, to which hydrochloric acid had been added to prevent the separation of oxide of tellurium.

The tellurium may be separated from this solution by means of metallic zinc; but it is then very impure, because it is depo-

sited along with all the metals precipitable by zinc, and mixed besides with those which the zinc itself contains. It is better to effect this precipitation by sulphurous acid gas; the pulverulent tellurium is then melted in a crucible to a regulus.

In order to obtain chemically pure tellurium, it is necessary to distil the tellurium precipitated by sulphurous acid in a stream of hydrogen. It is then melted in a crucible. Tellurium forms thus a regulus of a metallic lustre, which is the whiter the purer it is, and presents on its surface crystalline ramifications in the shape of fern leaves, like those exhibited by antimony. Its specific gravity, according to M. Löwe, is 6·18.

I made experiments, first on tellurium simply precipitated by sulphurous acid, and then on that distilled in a current of hydrogen.

*Tellurium not distilled.*

| | | | | | | |
|---|---|---|---|---|---|---|
| M | . | . | $167^{gr}·34$ | $167^{gr}·34$ | $167^{gr}·34$ | $167^{gr}·34$ |
| T | . | . | $97°·57$ | $98°·21$ | $98°·17$ | $98°·06$ |
| $\theta'$ | . | . | $20°·00$ | $21°·48$ | $22°·27$ | $20°·66$ |
| $\Delta\theta'$ | . | . | $1°·5988$ | $1°·5680$ | $1°·5505$ | $1°·5752$ |
| A | . | . | $422^{gr}·54$ | $422^{gr}·54$ | $422^{gr}·54$ | $422^{gr}·54$ |
| $c$ | . | . | $0·05205$ | $0·05160$ | $0·05158$ | $0·05136$ |

Mean 0·05160 (which is identical with that which I found in my former experiments).

The product of the specific heat by the atomic weight 806·5 is 41·65.

*Distilled Tellurium.*

| | | | | | | |
|---|---|---|---|---|---|---|
| M | . | . | . | . | $148^{gr}·15$ | $148^{gr}·15$ |
| T | . | . | . | . | $98°·28$ | $98°·33$ |
| $\theta'$ | . | . | . | . | $17°·88$ | $18°·92$ |
| $\Delta\theta'$ | . | . | . | . | $1°·3384$ | $1°·3191$ |
| A | . | . | . | . | $422^{gr}·54$ | $422^{gr}·54$ |
| | . | . | . | . | $0·04748$ | $0·04727$ |

Mean . . . . . 0·04737.

The product of this specific heat by the atomic weight is 38·20.

The latter result ought to be received; we see that it is comprised within the limits of variation observed in very pure simple bodies.

### Selenium.

I formerly determined the specific heat of selenium by uniting the specimens which were then to be found in the various collections in Paris. The substance was in the form of small pencils, or of medallions of Berzelius. I found for this specific heat the number 0·08371, which, multiplied by the atomic weight 491, gives the product 41·097.

The exhibition of M. Batka of Prague contained a large quan-

tity of selenium. I seized on this opportunity to make some
new experiments, which have led me to observe many curious
properties of this body. This selenium is not quite pure, it con-
tains about 2 per cent. of tellurium; but this quantity is far too
small to modify the physical properties of the body. M. Salvétat
obtained from an analysis the following results:—

| | |
|---|---|
| Tellurium | 2·25 |
| Sulphur | traces |
| Selenium (from the difference) | 97·75 |
| | 100·00 |

Berzelius thus describes the physical properties of selenium
(second edition of the French translation, vol. ii. p. 189):—
" When selenium cools after having been distilled, it assumes a
mirror-like surface, of a deep colour, verging towards a reddish-
brown, and having a metallic lustre rather like polished hæma-
tite. The fracture is conchoidal, vitreous, having a leaden-gray
colour and a metallic lustre. When allowed to cool slowly after
having been melted, its surface becomes unequally granular, of
a leaden-gray, and ceases to reflect. It has a fine-grained frac-
ture, is rough, and the mass is extremely like a fragment of
metallic cobalt. Fusion, followed by a sudden cooling, destroys
this appearance, and gives to selenium the characters which I
described at first. . . . . When selenium is reduced to powder, it
becomes of a deep red, but has a strong tendency to collect
together at various points : the friction of the pestle imparts to
it a polish, and turns it gray, as is the case when bismuth and
antimony are powdered. In thin plates selenium is transparent,
and has a deep ruby-red colour. It is softened by heat, becomes
semifluid at 100 degrees, and fuses completely a few degrees
above. In cooling, it remains for some time soft, and may then,
like sealing-wax, be drawn into slender and very flexible threads,
which, when flattened slightly, care being taken to keep them
soft, show better than any other form the transparency of sele-
nium. In reflected light these threads are gray, and have a
metallic lustre; viewed by daylight they are transparent, and of
a ruby-red. . . . . . . I found its specific weight to vary between
4·30 and 4·32. A slow cooling and a grained fracture have no
influence on its density."

According to M. Sacc (*Ann. de Chim. et de Phys.* 3rd series,
vol. xxi. p. 120), selenium softens at 200, and melts completely
at 250 degrees. When cooled, it commences to become viscous
at 150 degrees ; at 200 degrees it ceases to adhere to the bulb of
the thermometer; at 155 degrees it sticks to the sides of the
vessel, and solidifies completely at 150 degrees ; from whence
we may conclude that its melting-point is 200 degrees, and that

at which it solidifies 150 degrees. Nevertheless as this last cannot be fixed with precision, because selenium remains a long time pasty, we must believe that the point of solidification of selenium is at 200 degrees, like its melting-point, and that it is represented by the moment at which melted selenium ceases to adhere to the bulb of the thermometer which is plunged in it.

The characters which these two chemists assign to selenium are not very precise. Experiments which I have made on a mass of selenium weighing 250 grms. have allowed me to fix them better*.

Selenium exists, in fact, in two isomeric modifications; the *vitreous modification* and the *metallic modification*. In these two states selenium presents very distinct physical properties; and the passage from the one to the other is attended by very curious phænomena, the production of which is not difficult.

When selenium melts to a state of perfect liquidity, and is cast in a small trench-shaped mould formed of thin sheet brass, or in a glass tube, it solidifies in the form of a black mass with a brilliant surface, the fracture of which is exactly like that of black glass or of obsidian, and which has by no means a metallic aspect. This vitreous mass presents, when transparent, the colour of ruby-red in very fine splinters. Its dust remains gray under the pestle, and the red colour is not developed even by rubbing the pestle on a sheet of paper. I have always obtained selenium in this vitreous state, when, after having been melted, I have even allowed it to cool slowly.

But if selenium be so heated that its temperature rises very slowly, at the moment at which the thermometer indicates 96 to 97 degrees, the temperature rises suddenly with great rapidity, and in a few minutes it exceeds 200 to 230 degrees. If the substance is then examined, it is seen that its physical condition has been completely changed. Its surface presents a bluish-gray colour, and it has a fresh metallic lustre. Its fracture, instead of being vitreous, is in very fine metallic grains, quite resembling that of gray cast iron. Under the hammer the mass flattens perceptibly before breaking, and the fracture has then very decided metallic lustre. The file likewise produces surfaces having

* At the time of publishing this memoir, I was acquainted with no other researches than those I have named. Since then I have become acquainted with a memoir, read by M. Mitscherlich at the Academy of Sciences in Berlin in June 1855; that is at the time at which I was engaged on these experiments. A translation of this memoir is printed in the *Annales de Chimie et de Physique* for March 1856. It will be seen there, that M. Rittorf had observed before me, that selenium disengages heat at the moment of its molecular transformation; but the quantity of heat which he has given is far less than that which I have found in my experiments. Notwithstanding this, I have thought better to preserve the original text, which gives my experiments in the order in which they were made.

a metallic polish. In short, selenium exhibits, in this modification, the metallic character as decidedly as tellurium.

Vitreous selenium may be maintained many hours at a temperature of 90 degrees without undergoing the isomeric modification.

Metallic selenium is a better conductor of heat than vitreous selenium. This fact is easy of demonstration when the specific heat is determined; with vitreous selenium, the time required before the thermometer of the calorimeter attains its maximum temperature is two or three times as long as with metallic selenium.

Some selenium, melted at a temperature of 300 degrees, was thrown into very cold water; it solidified in large intertwined threads, having a very hard and vitreous fracture. The substance exhibited the same aspect as when allowed to cool in the air after fusion.

Vitreous selenium does not lose its characteristic properties by being pulverized. I placed selenium, recently powdered, in a glass tube, in the axis of which was a mercurial thermometer, and arranged the tube in the oven for specific heats kept at a temperature of 100 degrees. When the thermometer had reached 97 degrees, it mounted suddenly and with great rapidity until it reached 160 degrees. The substance had changed its aspect : it had agglomerated, and had assumed the metallic appearance.

Selenium, precipitated in the state of red powder from its solutions, belongs to the vitreous modification. To assure myself of this, I dissolved 50 grms. of selenium in nitric acid; then after having evaporated it to dryness and redissolved it in water, I precipitated the selenium by a current of sulphurous acid. The precipitate, quickly washed with water, was dried *in vacuo*. The pulverulent selenium was heaped up in a glass tube round the bulb of the thermometer, and the tube placed in the oven heated to 100 degrees. At about 94 degrees the thermometer assumed a very rapid upward march till it reached 150 degrees. The substance was aggregated together in several metallic gray masses, and had separated from the thermometer. In consequence of the latter circumstance, the thermometer had not reached a higher point.

The following are the experiments which I made on the specific heat of metallic selenium :—

| M | . | . | $112^{gr}\cdot20$ | $112^{gr}\cdot20$ | $200^{gr}\cdot90$ | $200^{gr}\cdot90$ |
|---|---|---|---|---|---|---|
| T | . | . | $98^{\circ}\cdot13$ | $98^{\circ}\cdot33$ | $96^{\circ}\cdot08$ | $97^{\circ}\cdot69$ |
| $\theta'$ | . | . | $21^{\circ}\cdot25$ | $23^{\circ}\cdot88$ | $20^{\circ}\cdot14$ | $18^{\circ}\cdot49$ |
| $\Delta\theta'$ | . | . | $1^{\circ}\cdot9454$ | $1^{\circ}\cdot8952$ | $3^{\circ}\cdot4289$ | $3^{\circ}\cdot2543$ |
| A | . | . | $422^{gr}\cdot54$ | $422^{gr}\cdot54$ | $422^{gr}\cdot54$ | $422^{gr}\cdot54$ |
| $c$ | . | . | $0\cdot07517$ | $0\cdot07563$ | $0\cdot7675$ | $0\cdot7709$ |

Mean . . . . $0\cdot07616$.

The atomic weight of selenium is 491. The product of the

mean specific heat by the atomic weight is 37·39. This product is comprised within the limits which we have found for other simple bodies.

To determine the specific heat of vitreous selenium, I could not use the ordinary apparatus, because the substance undergoes its isomeric transformation at 93 degrees. I used the apparatus represented by fig. 2, the trough being filled with hot water. The water of this trough was kept stationary by means of a gas lamp; it was constantly agitated by means of the palette agitator $m\,n\,p\,q$.

Selenium recently melted was cast in the small brass vessel, fig. 1; this vessel was placed in the middle of the tube A B, where it was maintained by the help of two silk threads attached to the upper and lower base of the vessel, in order to move it readily in lowering the thermometer.

| | | |
|---|---|---|
| Weight of selenium | $188^{gr}\!\cdot\!5$ | $185^{gr}\!\cdot\!6$ |
| Weight of brass vessel | $81^{gr}\!\cdot\!2$ | $81^{gr}\!\cdot\!2$ |
| T | $87°\!\cdot\!285$ | $77°\!\cdot\!338$ |
| $\theta'$ | $19°\!\cdot\!49$ | $17°\!\cdot\!93$ |
| $\Delta\theta'$ | $4°\!\cdot\!4333$ | $3°\!\cdot\!7614$ |
| A | $422°\!\cdot\!54$ | $422°\!\cdot\!54$ |
| $c$ | $0\!\cdot\!1036$ | $0\!\cdot\!1026$ |
| Mean | $0\!\cdot\!1031.$ | |

The specific heat which we have thus found for vitreous selenium is much greater than that which we have obtained for metallic selenium, which only amounts to 0·07616. But this may arise from the fact that vitreous selenium is much more easily softened by heat than metallic selenium, and that at the temperature of 80 or 90 degrees it already contains a considerable part of its latent heat of fusion.

In order to decide if selenium possesses different specific heats in its two isomeric states, it was necessary to determine them at very low temperatures, at which neither of the two varieties of selenium undergoes a perceptible softening during the period of the temperature to which they are subjected. The selenium was previously melted in the ordinary mould; it was then in the form of a large cylinder, having in the direction of its axis an aperture in which was placed the bulb of the mercurial thermometer employed to indicate the temperature. The cylinder attached to its two silk threads was kept in the middle of the tube A B, fig. 2; and the vessel C D E F was filled with a cooling mixture formed of ice and crystallized chloride of calcium, which was continually agitated. The cooling of the thermometer was followed till it reached its minimum, at which it usually remained ten minutes. At this moment the calorimetric experiment was made. The following are the results obtained with vitreous selenium and with metallic selenium :—

|   | Vitreous selenium. | | Metallic selenium. | |
|---|---|---|---|---|
| P . . | 209gr·615 | 209gr·615 | 209gr·57 | 209gr·57 |
| T . . | −27°·408 | −20°·516 | −20°·270 | −15°·852 |
| θ' . . | 8°·441 | 6°·99 | 7°·019 | 6°·774 |
| Δθ' . . | 1°·3254 | 1°·0202 | 0°·9911 | 0°·8496 |
| A . . | 422gr·54 | 422gr·54 | 422gr·54 | 422gr·54 |
| c . . | 0·07461 | 0·7476 | 0·07323 | 0·07570 |

Means . . 0·07468.    0·07446.

Hence at low temperatures the two isomeric modifications of selenium have exactly the same calorific capacity. This result is similar to that which I obtained with the two isomeric modifications of phosphorus, ordinary phosphorus, and red phosphorus. In two such different conditions, phosphorus showed calorific capacities which were evidently equal (*Ann. de Chim. et de Phys.* 3rd series, vol. xxxviii. p. 129).

Vitreous arsenious acid and opake arsenious acid also exhibit no difference in their specific heats. It is nevertheless remarkable that the enormous disengagement of heat which is manifested at the moment at which vitreous selenium is transformed into metallic selenium, should not produce any change in the calorific capacity of this body.

Selenium presents no fixed point of fusion or of solidification; when heated, it softens gradually, and only becomes completely liquid at a temperature above 250 degrees. Every one knows that in its viscous state it may be drawn out in very fine threads. I melted a great mass of selenium in a large glass tube, and kept in the middle of the liquid a mercurial thermometer, and followed from minute to minute the cooling of the thermometer. From 260 to 40 degrees the decrease of the thermometer was perfectly regular; at no point was there remarked one of those intervals of delay, more or less prolonged, which are always observed at the moment of a change of condition in bodies which pass from the liquid to the solid state.

I give here the series of temperatures observed from minute to minute:—

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 0 | . . . | 201·2 | 15·5 | | 9 | . . . | 118·7 | 5·8 | |
| 1 | . . . | 185·7 | 13·35 | | 10 | . . . | 113·3 | 5·4 | |
| 2 | . . . | 172·35 | 9·85 | | 11 | . . . | 108·0 | 5·3 | |
| 3 | . . . | 162·5 | 9·3 | | 12 | . . . | 103·4 | 4·6 | |
| 4 | . . . | 153·2 | 8·5 | | 13 | . . . | 98·5 | 4·9 | |
| 5 | . . . | 144·7 | 7·25 | | 14 | . . . | 94·25 | 4·25 | |
| 6 | . . . | 137·45 | 6·55 | | 15 | . . . | 90·25 | 4·0 | |
| 7 | . . . | 130·9 | 6·4 | | 16 | . . . | 86·4 | 3·85 | |
| 8 | . . . | 124·5 | | | 17 | . . . | 83·0 | 3·4 | |

In a second experiment, in order to render the cooling slower, I arranged the tube containing the melted selenium in the oven heated to 100 degrees. We have thus—

| ′ | | $°$ | $°$ | ′ | | $°$ | $°$ |
|---|---|------|------|---|---|------|------|
| 0 | . . . | 241·6 | 11·8 | 29 | . . . | 133·9 | 0·80 |
| 1 | . . . | 229·8 | 10·5 | 30 | . . : | 133·2 | 0·70 |
| 2 | . . . | 219·3 | 9·1 | 35 | . . . | 129·2 | 0·80 |
| 3 | . . . | 210·2 | 8·5 | 40 | . . . | 125·35 | 0·77 |
| 4 | . . . | 201·7 | 7·3 | 45 | . . . | 121·9 | 0·69 |
| 5 | . . . | 194·4 | 6·8 | 50 | . . . | 119·1 | 0·60 |
| 6 | . . . | 187·6 | 6·0 | 55 | . . . | 116·8 | 0·46 |
| 7 | . . . | 181·6 | 5·65 | 60 | . . . | 115·0 | 0·36 |
| 8 | . . . | 175·95 | 5·05 | 65 | . . . | 113·40 | 0·32 |
| 9 | . . . | 170·9 | 4·5 | 70 | . . . | 112·80 | 0·12 |
| 10 | . . . | 166·4 | 4·0 | 75 | . . . | 112·60 | 0·04 |
| 11 | . . . | 162·4 | 3·6 | 80 | . . . | 112·55 | 0·00 |
| 12 | . . . | 158·8 | 3·2 | 85 | . . . | 113·0 | +0·09 |
| 13 | . . . | 155·6 | 2·7 | 90 | . . . | 114·05 | +0·21 |
| 14 | . . . | 152·9 | 2·5 | 95 | . . . | 115·9 | +0·37 |
| 15 | . . . | 150·4 | 2·3 | 100 | . . . | 117·9 | +0·40 |
| 16 | . . . | 148·1 | 1·8 | 105 | . . . | 120·75 | +0·57 |
| 17 | . . . | 146·3 | 1·6 | 110 | . . . | 121·3 | +0·11 |
| 18 | . . . | 144·7 | 1·45 | 115 | . . . | 118·9 | −0·48 |
| 19 | . . . | 143·25 | 1·25 | 120 | . . . | 116·0 | 0·58 |
| 20 | . . . | 142·0 | 1·25 | 125 | . . . | 113·35 | 0·53 |
| 21 | . . . | 140·75 | 1·10 | 130 | . . . | 111·25 | 0·42 |
| 22 | . . . | 139·65 | 0·90 | 135 | . . . | 109·65 | 0·32 |
| 23 | . . . | 138·75 | 0·90 | 140 | . . . | 108·45 | 0·24 |
| 24 | . . . | 137·85 | 0·85 | 145 | . . . | 107·55 | 0·18 |
| 25 | . . . | 137·0 | 0·75 | 150 | . . . | 106·8 | 0·15 |
| 26 | . . . | 136·25 | 0·75 | 155 | . . . | 106·3 | 0·10 |
| 27 | . . . | 135·5 | 0·80 | 160 | . . . | 105·7 | 0·12 |
| 28 | . . . | 134·7 | | 165 | . . . | 105·1 | 0·12 |

It will be remarked that the cooling proceeded almost regularly until the 55th minute, when the thermometer indicated 116°·8; the rapidity of the cooling diminished then in an anomalous manner till the 75th. From the 75th to the 80th the thermometer remained almost stationary; it then entered on an upward march until the 110th minute, where it showed 121°·3, a temperature almost 9 degrees higher than that which was indicated at the 80th. It then assumed anew an evidently regular descending march.

Thus the only anomaly remarked during the cooling took place at about 120 degrees; and the inverse march which the thermometer takes, proves that the solidification then makes rapid pro-

gress, or that a molecular movement is effected which disengages a considerable quantity of heat. But this is not the movement which transforms vitreous selenium into metallic selenium; for the mass of selenium taken out of the oven after the 165th minute still preserved its vitreous aspect in the fracture, even when quite cool.

### Experiments on the Heat disengaged by Selenium at the moment of its isomeric transformation.

I wished to determine as exactly as possible the quantity of heat which selenium disengages in undergoing its isomeric transformation in the oven heated to 100 degrees. To this end, I melted the selenium in the mould represented in fig. 1, only replacing the cylinder *ab* by a very fine brass tube open at both ends. The mould thus filled with selenium was suspended, when cool, in the oven heated to 100 degrees, the bulb of the thermometer of the oven being adjusted in the central tubulure. In order to prevent the thermometer from being too much cooled by the current of air which must necessarily be established through this tubulure at the moment at which selenium disengages heat, a lute of bread paste was spread on the inferior orifice. The following is the succession of temperatures observed:—

| | | | |
|---|---|---|---|
| 0 | . . . | 72°·83 | 0°·66 |
| 10 | . . . | 79·45 | 0·49 |
| 20 | . . . | 84·35 | 0·38 |
| 30 | . . . | 88·15 | 0·33 |
| 35 | . . . | 89·8 | 0·30 |
| 40 | . . . | 91·3 | 0·30 |
| 45 | . . . | 92·85 | 0·35 |
| 50 | . . . | 94·6 | 0·38 |
| 55 | . . . | 96·5 | 0·45 |
| 56 | . . . | 96·95 | 0·55 |
| 57 | . . . | 97·5 | 0·50 |
| 58 | . . . | 98·0 | 0·65 |
| 59 | . . . | 98·65 | 0·65 |
| 60 | . . . | 99·3 | 0·75 |
| 61 | . . . | 100·05 | 0·75 |
| 62 | . . . | 100·8 | 0·90 |
| 63 | . . . | 101·7 | 1·20 |
| 64 | . . . | 102·9 | 1·20 |
| 65 | . . . | 104·1 | 1·60 |
| 66 | . . . | 105·7 | 2·10 |
| 67 | . . . | 107·8 | 2·87 |
| 68 | . . . | 110·67 | |

| | | | |
|---|---|---|---|
| 69 | . . . | 115°·3 | 4°·63 |
| 70 | . . . | 124·6 | 9·3 |
| 71 | . . . | 148·5 | 23·9 |
| 71¼ | . . . | 161·4 | } 53·0 |
| 71½ | . . . | 177·2 | |
| 71¾ | . . . | 193·0 | |
| 72 | . . . | 201·5 | |
| 72¼ | . . . | 210·1 | |
| 72½ | . . . | 212·5 | } 13·5 |
| 72¾ | . . . | 213·3 | |
| 73 | . . . | 214·0 | |
| 73¼ | . . . | 214·2 | |
| 73½ | . . . | 214·2 | } −0°·1 |
| 73¾ | . . . | 214·1 | |
| 74 | . . . | 213·9 | |
| 75 | . . . | 212·0 | −1·9 |
| 76 | . . . | 208·0 | −4·0 |
| 77 | . . . | 200·8 | 7·2 |
| 78 | . . . | 192·6 | 8·2 |
| 79 | . . . | 185·2 | 7·4 |
| 80 | . . . | 179·1 | 6·1 |
| 81 | . . . | 173·4 | 5·7 |

| | | | | |
|---|---|---|---|---|
| 82 . . . 168·6 | 4·8 | 93 . . . 135·8 | 2·1 |
| 83 . . . 164·5 | 4·1 | 94 . . . 133·8 | 2·0 |
| 84 . . . 160·5 | 4·0 | 95 . . . 131·9 | 1·9 |
| 85 . . . 157·0 | 3·5 | 96 . . . 130·2 | 1·7 |
| 86 . . . 153·25 | 3·75 | 97 . . . 128·25 | 1·95 |
| 87 . . . 150·6 | 2·65 | 98 . . . 127·0 | 1·25 |
| 88 . . . 147·7 | 2·9 | 99 . . . 125·6 | 1·4 |
| 89 . . . 145·1 | 2·6 | 100 . . . 124·2 | 1·4 |
| 90 . . . 142·6 | 2·5 | 105 . . . 118·35 | 1·17 |
| 91 . . . 140·1 | 2·5 | 110 . . . 113·95 | 0·88 |
| 92 . . . 137·9 | 2·2 | 115 . . . 110·6 | 0·67 |
| | | 120 . . . 108·0 | 0·52 |

It is seen from this table that up to 93 degrees the thermometer rose constantly with a decreasing rapidity, as ought to be the case, for it was thus approximating to the surrounding temperature, which was 100 degrees; but from 94 degrees the ascending march of the thermometer was accelerated, at first feebly, but afterwards with rapidity. At the 61st minute the thermometer exceeded the surrounding medium. From the 71st to the 72nd minute it increased 53 degrees; its ascending march was then somewhat slower, and it was at the 73rd minute that it reached its maximum of 214 degrees. From thence the thermometer assumed a descending march, which was at first accelerated up to the 78th minute, because the substance still emitted heat which partially compensated for the effect of the cooling causes : at last the rapidity of the cooling abated, the excess of temperature becoming less and less. It is besides evident that this superelevation of temperature of more than 114 degrees, is far less than that which would have taken place if the disengagement of heat, in consequence of the molecular transformation, instead of being successive, had been instantaneous, and if the substance had not been obliged to divide its heat with foreign bodies, and to lose it by radiation.

Thus in the experiment before us, the weight of selenium is 200 grammes. Assuming that its specific heat is 0·0769, as we have found it between 15 and 90 degrees, this selenium corresponds, in its calorific capacity, to 15$^{gr}$·42 of water. The brass vessel which contains it weighs 81$^{gr}$·15, and corresponds to 7$^{gr}$·70 of water. We may assume that this case is at the same temperature as the selenium which it contains. In this case if $x$ represents the temperature to which selenium would have been raised had it been alone, we should have

$$15·42 \, . \, x = (15·42 + 7·70) \, . \, 114, \text{ whence } x = 170°·9.$$

Thus the temperature would have been raised 171 degrees if the

selenium had been alone; and we still neglect the heat which our complex system has lost by radiation in the oven from the 61st minute, at which it began to exceed the temperature of the oven, to the 73rd minute, at which it reached its maximum temperature.

It cannot be admitted that at the moment at which the thermometer indicates its maximum temperature, the molecular transformation is complete, and that the calorific source ceases to act; for from the 73rd to the 78th minute the rapidity of the cooling increased, which proves that a disengagement of heat constantly decreasing was opposed to the normal cooling of the system. If we assume that the heating cause had ceased at the 78th minute, and that from this time the cooling took place proportionally to the excess of the temperature of the system above the surrounding medium, we could then calculate approximatively the temperature which the system has lost by radiation from the 61st minute to the 73rd. The loss of temperature during each minute from the 72nd to the 78th would have been at least $8°\cdot2$ if the body had not disengaged any heat. But the variation of temperature was—

$$
\begin{aligned}
\text{From } 72' \text{ to } 73' &+ 13\overset{\circ}{\cdot}5 \\
73 \text{ to } 74 &- 0\cdot1 \\
74 \text{ to } 75 &- 1\cdot9 \\
75 \text{ to } 76 &- 4\cdot0 \\
76 \text{ to } 77 &- 7\cdot2 \\
77 \text{ to } 78 &- 8\cdot2
\end{aligned}
$$

Comparing these coolings with that of $8°\cdot2$, which would have been the normal cooling, we see that there ought to have been disengaged—

From 72′ to 73′ a quantity of heat which would have raised the thermometer, if it had not been compensated for by the cooling . . . . . . . . . . . . . . . $8\cdot2$

From 73′ to 74′ a quantity which would have raised the thermometer to ,, . . . . $8\cdot1$

From 74′ to 75′ ,, ,, . . . . $6\cdot3$

From 75′ to 76′ ,, ,, . . . . $4\cdot2$

From 76′ to 77′ ,, ,, . . . . $1\cdot0$

From 77′ to 78′ ,, ,, . . . . $0\cdot0$

Number of degrees by which the temperature would have been increased up to the 78th minute, if there had been no cooling by radiation . . . . . . . . . . . $27\cdot8$

We may now assume, supposing that the loss of heat by radiation was, during the ascending period of the thermometer, equal to that observed during the descending period, for equal

excesses of temperature of the system above the surrounding medium :—

|  | | | | | Mean temperature. | Cooling. |
|---|---|---|---|---|---|---|
| From 71 to 72 | . | . | . | 175·5 | 5·7 |
| 70 to 71 | . | . | . | 136·6 | 2·1 |
| 69 to 70 | . | . | . | 120·0 | 1·0 |
| 68 to 69 | . | . | . | 113·0 | 0·7 |
| 67 to 68 | . | . | . | 109·2 | 0·5 |
| 66 to 67 | . | . | . | 106·8 | 0·34 |
| 65 to 66 | . | . | . | 105·0 | 0·25 |
| 64 to 65 | . | . | . | 103·5 | 0·17 |
| 63 to 64 | . | . | . | 102·3 | 0·12 |
| 62 to 63 | . | . | . | 101·3 | 0·07 |
| 61 to 62 | . | . | . | 100·5 | 0·02 |

(*) applies to the group from 66 to 67 through 61 to 62.

10·97

Thus the system would have lost by radiation, during the ascending period of the thermometer, 10°·97. By these two causes, the total elevation of the thermometer was too little by 27°·8 + 10°·97, or by 38°·77.

Hence, if the losses by radiation had not existed, the thermometer would have been raised to 214 + 38·77, that is to 253 degrees; and if the selenium had not been enclosed in an envelope of brass, which divided with it the heat emitted, the elevation of temperature experienced above 100 degrees would have been $x$, given by the formula

$$15·42x = 23·12 \cdot 153, \text{ whence } x = 229°.$$

According to this calculation, which is only approximative, the temperature of dry selenium would have been raised from 98 to 329 degrees in consequence of the molecular transformation, if all the heat disengaged had been effective.

I sought to determine, in another manner, the quantity of heat which selenium disengages during its molecular transformation, and made the following experiment :—

The small vessel of brass containing the selenium recently melted was placed in the oven for specific heats, exactly as in the preceding experiment. When the thermometer of the oven began to rise above 100 degrees, in consequence of the molecular transformation, the calorimeter for specific heats was arranged, and at the moment at which the thermometer of the oven exhibited the maximum, the small vessel was rapidly lowered into

* As the cooling was not observed in the second period under 108 degrees, the last partial coolings were calculated by means of the formula $\Delta\theta = A\theta$; $\theta$ representing the excess of the temperature above 100 degrees. The constant was calculated according to the observation (69 to 70), that is, 1·00 was assumed to be equal to A 20, whence $A = \frac{1}{40}$.

the calorimeter. In this manner the total quantity of heat could be determined which the system possessed at the moment at which the thermometer of the oven indicated its maximum.

The following are the data of the calorimetric experiment :—

| | | |
|---|---|---|
| Weight of selenium . . . . . | $200^{gr}\cdot45$ | |
| Weight of brass vessel . . . . . | $81^{gr}\cdot15$ | Value in |
| Maximum reached by the thermo- | | water $7^{gr}\cdot62$. |
| meter of the oven . . . . . . | $200°\cdot5$ | |
| Final temperature of the calorimeter | $24°\cdot65$ | |
| Elevation of temperature after cor- | | |
| rection . . . . . . . . . | $9°\cdot115$ | |
| A . . . . . . . . . . . . | $422^{gr}\cdot54$ | |

The quantity of heat acquired by the calorimeter, and which is equal to that which the system gave up in setting itself in equilibrium with it, that is, in decreasing to $24°\cdot65$, is

$$422\cdot54 \cdot 9°\cdot115 = 3851\cdot5 \text{ units of heat.}$$

This quantity of heat is composed of two parts :—

1. Of that which would have been imparted to it by the oven, if the selenium had not spontaneously disengaged heat.

2. Of the quantity of heat which the molecular transformation of the selenium liberated.

It is the latter quantity of heat which it is important to know ; we shall obtain it by subtracting from the total quantity of heat given by the experiment, that which vitreous selenium gives up when it cools from 98 degrees (the maximum to which bodies usually reach in our oven) to $24\cdot65$. But we have found that the specific heat of vitreous selenium is $0\cdot1036$ between $19°\cdot5$ and $87°\cdot3$; if we assume that it is the same between $24°\cdot65$ and $98°\cdot0$, the augmentation which the specific heat may undergo from 87 to 98 degrees being attributable to the molecular transformation, selenium in falling from 98 to $24\cdot65$ would abandon a quantity of heat represented by

$$200\cdot45 \times 0\cdot1036 \times 73\cdot35 = 1523\cdot2$$

The envelope of brass in cooling the same $\left.\begin{array}{l}\text{number of degrees } 7\cdot62 \times 73\cdot35 \quad . \quad .\end{array}\right\} = \dfrac{558\cdot9}{2082\cdot1}$

We obtain therefore for the number of units of heat disengaged by the molecular transformation, $3851\cdot5 - 2082\cdot1 = 1769\cdot4$, which have been produced by $200^{gr}\cdot45$ of selenium. This number would heat $200^{gr}\cdot45$ of selenium, supposing it to be the metallic modification by $\dfrac{1769\cdot4}{200\cdot45\cdot0\cdot076}$, or by $116\cdot00$, which differs little from the $114\cdot2$ which we have observed directly on the thermometer of the oven at the moment at which it indicated the maximum. But like the last, it must undergo im-

portant corrections; for during all the time which the ther-
mometer has taken to rise from 93 degrees to its maximum, the
system has lost by radiation a considerable portion of the heat
which would become free in consequence of the molecular trans-
formation. To be able to calculate this loss of heat, I transcribe
the table of observations of temperatures made before the time
at which the selenium was lowered into the calorimeter.

| Variation in minutes. | | | Variation in minutes. | | |
|---|---|---|---|---|---|
| 0 . . . | 72·83 | 0·63 | 69 . . . | 96·1 | 0·35 |
| 5 . . . | 76·00 | 0·52 | 70 . . . | 96·45 | 0·35 |
| 10 . . . | 78·6 | 0·48 | 71 . . . | 96·7 | 0·25 |
| 15 . . . | 81·0 | 0·35 | 72 . . . | 97·2 | 0·50 |
| 20 . . . | 82·9 | 0·35 | 73 . . . | 97·7 | 0·50 |
| 25 . . . | 84·65 | 0·33 | 74 . . . | 98·05 | 0·55 |
| 30 . . . | 86·3 | 0·28 | 75 . . . | 98·5 | 0·45 |
| 35 . . . | 87·7 | 0·25 | 76 . . . | 99·1 | 0·60 |
| 40 . . . | 88·95 | 0·23 | 77 . . . | 99·7 | 0·60 |
| 45 . . . | 90·1 | 0·22 | 78 . . . | 100·3 | 0·60 |
| 50 . . . | 91·2 | 0·24 | 79 . . . | 101·0 | 0·70 |
| 55 . . . | 92·4 | 0·15 | 80 . . . | 101·95 | 0·95 |
| 56 . . . | 92·55 | 0·25 | 81 . . . | 103·1 | 1·15 |
| 57 . . . | 92·8 | 0·20 | 82 . . . | 104·15 | 1·05 |
| 58 . . . | 93·0 | 0·20 | 83 . . . | 105·7 | 1·55 |
| 59 . . . | 93·2 | 0·25 | 84 . . . | 107·8 | 2·10 |
| 60 . . . | 93·45 | 0·35 | 85 . . . | 110·9 | 3·10 |
| 61 . . . | 93·8 | 0·30 | 86 . . . | 116·5 | 5·60 |
| 62 . . . | 94·1 | 0·30 | 87 . . . | 127·6 | 11·10 |
| 63 . . . | 94·4 | 0·20 | 88 . . . | 147·6 | 20·00 |
| 64 . . . | 94·6 | 0·20 | 89 . . . | 187·0 | 39·40 |
| 65 . . . | 94·8 | 0·35 | 90 . . . | 200·4 | 13·4 |
| 66 . . . | 95·15 | 0·30 | | | |
| 67 . . . | 95·45 | 0·30 | | | |
| 68 . . . | 95·75 | | | | |

As the thermometer now rose only
slowly, the system was lowered; it
was placed in the calorimeter at the
92nd minute.

We see here again that the rapidity of the heating goes
on diminishing up to the 58th minute, when the thermometer
shows 93 degrees; from the 58th to the 65th minute, this rapidity
remains perceptibly constant, assuming then an ascending march.
We may therefore suppose then that the molecular transform-
ation commences at 93 degrees, and that selenium, during its
passage from 93 to 98 degrees, is only heated by the radiation
of the enclosure. If it be admitted that the molecular transform-
ation has not commenced from 76 to 93 degrees, we might, as-
suming Newton's law, calculate from the numbers of this period
the heat which the enclosure has communicated to the system,

We may put $\Delta\theta = A\theta$, $\theta$ representing the excess of temperature of the enclosure, and $\Delta\theta$ the increase of temperature which the system obtains in 1 minute. To calculate the constant A, we might put, according to the observation from 5 to 10 minutes,—

$$0\cdot48 = A(98 - 77\cdot3) = A(20\cdot07),$$

$$\text{whence } A = \frac{0\cdot48}{20\cdot7} = 0\cdot0232.$$

We may calculate from this formula the elevation of temperature which the enclosure has given in each minute.

From the 78th minute, when the system exceeds 100 degrees, to the 92nd minute, when it is plunged into the water of the calorimeter, it loses necessarily a certain quantity of heat, in consequence of its excess of temperature above the enclosure, which does not exceed 100 degrees. We proceed to estimate this loss of temperature from the observations of cooling, which followed the molecular transformation in the experiment of page 513. We shall thus obtain

|  |  | Mean temperature. | Cooling. |
|---|---|---|---|
| From 78 to 79 . . . | | 100·65 | 0·02 |
| 79 to 80 . . . | | 101·47 | 0·07 |
| 80 to 81 . . . | | 102·50 | 0·12 |
| 81 to 82 . . . | | 103·6 | 0·17 |
| 82 to 83 . . . | | 104·9 | 0·25 |
| 83 to 84 . . . | | 106·8 | 0·34 |
| 84 to 85 . . . | | 109·4 | 0·50 |
| 85 to 86 . . . | | 113·7 | 0·70 |
| 86 to 87 . . . | | 122·0 | 1·10 |
| 87 to 88 . . . | | 137·6 | 2·20 |
| 88 to 89 . . . | | 167·3 | 4·20 |
| 89 to 90 . . . | | 193·7 | 8·00 |
| 90 to 91 . . . | | 200·0 | 8·40 |
| 91 to 92 . . . | | 200·0 | 8·40 |
|  |  |  | 34·47 |

Thus the system ought to have lost by radiation about 34°·5 of temperature; and the final temperature would have risen as much if this loss had not taken place. But we observe that 34°·47 of temperature lost by the system corresponds to a number of units of heat represented by

$$(200\cdot45 \times 0\cdot1036 + 7\cdot62)34\cdot47 = 978\cdot6,$$

which, added to 3851·5, which the calorimeter has given directly, furnishes a total of 4830·1.

If we subtract the 2082·1 units of heat, which we may suppose

to have been communicated to the system by the enclosure, we obtain 2748 units of heat, which would raise $200^{gr.}45$ of metallic selenium from 0 degree to the temperature

$$\frac{2748}{200 \cdot 45 \cdot 0 \cdot 0761} = 180^{\circ} \cdot 15.$$

This elevation of temperature is less than that which we have found by the first mode of calculation; but I shall show that it is necessarily too little, for in the preceding calculation we have assumed by inference, that at the moment at which the thermometer marked the maximum in the oven, and at which it was plunged into the calorimeter, the molecular transformation was complete. But we know from the first experiment, that even then the substance still continued to disengage heat, and we allowed for that in our first calculation.

In recapitulating, I think we may conclude from these experiments that vitreous selenium disengages during its transformation into metallic selenium a quantity of heat, which would elevate the latter by more than 200 degrees.

I formerly found (*Ann. de Ch. et de Phys.* 3rd series, vol. i. p. 205) an analogous disengagement of heat during the transformation of soft sulphur into ordinary sulphur, but the quantity of heat liberated was much less, for it only raised the temperature of the substance 12 to 14 degrees. It is true that the sulphur is then brought almost to its fusing-point, so that part of the heat disengaged may be disguised by partial fusion.

LXVI. *On an Improved Construction of* Ruhmkorff's *Induction Coil. By* C. A. BENTLEY, *Esq.*

*To the Editors of the Philosophical Magazine and Journal.*

128 Sloane Street, Chelsea,
GENTLEMEN,                  Dec. 3, 1856.

AS many of your readers may be interested in the production of induction coils, I take the liberty of sending a short, but, I trust, concise description of one I have constructed; and as it is the result of two years' experience, it will no doubt prove valuable to those interested in electro-magnetic induction.

The instrument referred to gives an induction spark 2 inches long in air of ordinary density when excited by four or five cells of Grove's battery; and the quantity of static electricity is so great, that it will charge a quart Leyden jar two hundred times per second, the discharge taking place through an interval of $1\frac{1}{4}$ inch.

The thermal phænomena are also very striking; for when the secondary wires are separated about three-quarters of an inch,

an arc of flame passes the interval, and fuses electrodes present-
ing twenty times the sectional area of the wire from which the
current is produced: the flame can at the same time be acted
upon by a permanent magnet in the same manner as the voltaic
arc. This heating power appears to depend, not upon the *quan-
tity* of electricity passing, but upon the resistance it can over-
come; consequently the thermal effects disappear *in vacuo*, to be
reproduced upon the gradual admission of air or other resisting
media.

My induction apparatus differs from Ruhmkorff's in three
important parts of its structure: first, in the method of insu-
lation; secondly, in the contact breaker; and thirdly, in the
formation of the condenser. Ruhmkorff, as your readers are
aware, insulated his secondary wire with shell-lac; but as this
substance, though an excellent insulator, is liable to crack, I have
adopted gutta-percha tissue for the insulation of my wire; and
I apply it in the form of ribbon an inch wide, which I obtain by
cutting slices from a firm roll of that substance; four or five
layers are necessary between each layer of wire, as the current
from a single layer is sufficiently intense to strike through one-
tenth of an inch of air.

The contact breaker is a most important part of the apparatus,
for without the one I have contrived I do not obtain nearly so
great a quantity of static electricity. It consists of a strong
steel spring, fastened firmly at one end, and having the platinum
contact piece in the centre, behind which is a piece of iron to be
attracted by the iron core; at the other end of the spring is a
screw, by which I can force the two contact pieces together with
a force of one ounce to ten pounds; the steel spring therefore
vibrates from its centre, and it is only when the whole of the
battery current has traversed the primary wire that the iron core
has sufficient power to draw the two contact pieces asunder. It
may be interesting to state, that the condenser has not the slight-
est effect on the *quantity* of electric force developed in the second-
ary wire, but increases the intensity to an enormous extent; and
as it is better to have it as large as possible, I have formed mine
of 120 sheets of tinfoil, 6 × 12, placed between double that num-
ber of varnished sheets of paper, the alternate sheets of foil being
brought out and soldered to appropriate binding-screws. The
length of secondary wire in the coil described is 2 miles, and its
gauge No. 35. The primary helix is formed of 30 yards of
No. 14 wire, and is wound on an iron wire core 9 inches long
and $1\frac{3}{4}$ inch diameter, in which is placed as an axis an iron rod
to support the coil, which I place in a box constructed for that
purpose.

Such, then, is the very slight description I have to offer of an

instrument which produces a spark five times the length of those produced by any of Ruhmkorff's arrangements; but as he was certainly the originator of these instruments, and therefore deserves nearly all the credit, I hope that mine and those produced by others will still be called Ruhmkorff's coils.

Yours obediently,

C. A. BENTLEY.

LXVII. *On the Transmutation of Rays of Light ; with a Reply to the " Remarks " of* Professor Stokes *in the December Number. By* Professor CHALLIS*.

BEFORE replying to the Remarks which Professor Stokes has made on certain parts of my communication in the November Number, entitled " A Theory of the Composition of Colours," I am desirous of entering upon some theoretical considerations by which the views there advanced, and the arguments by which I support them, will be better understood.    I commence with saying, that, consistently with all I have hitherto written on the undulatory theory of light, I shall suppose the æther to be a continuous medium so constituted that variations of its pressure are proportional to variations of its density, and shall admit for the explanation of phænomena of light only such properties of the medium as may be deduced from this constitution by calculating on hydrodynamical principles.    Also for the present purpose it will suffice merely to enunciate those deductions which bear upon the phænomena of light with which this communication is immediately concerned, and to mention that proofs of them, which I am prepared to maintain, have been given in articles contributed at various times to this Journal.

When a medium, constituted as above supposed, suffers a continuous vibratory disturbance by the motions of the atoms of any extraneous substance, or when a state of continuous vibratory motion of the æther suffers disturbance by the passive action of atoms at rest, the consequences, as deduced by the analysis, are the following :—

(1) The state of the disturbed æther at each instant, both as to velocity and condensation, results from the composition of an indefinite number of separate motions, such that each has an axis of symmetry parallel and transverse to which vibrations take place according to a law expressed by the function

$$m \sin \frac{2\pi}{\lambda} (x - at + c).$$

* Communicated by the Author.

522      Prof. Challis *on the Transmutation of Rays of Light.*

(2) The compound character of the motion, and the form of the above function, are independent of the nature of the disturbance.

(3) The number of the separate motions, the directions of their axes, and the values of the arbitrary constants $m$, $\lambda$, and $c$, must be such as to satisfy the circumstances of the disturbance, and will be different for different kinds of disturbance.

(4) When the æther in motion suffers disturbance by encountering atoms actually or relatively at rest, and the original motion is a simple series of vibrations of the kind above described, or is compounded of several such motions with parallel axes and different values of $m$, $\lambda$ and $c$, the result of the disturbance may in either case consist of an indefinite number of separate motions having their axes in various directions, and having values of $m$, $\lambda$, and $c$ altogether different from the values of these quantities in the original motion. This general result includes the particular cases in which the axes of the new vibrations are parallel to the common direction of those of the original motions, or deviate from it by small angular inclinations.

(5) The limits of the values of $\lambda$ in the disturbed state of the medium depend on the circumstances of the disturbance; and when the disturbance is of the kind mentioned in (4), are not necessarily the same as the limits of the values of $\lambda$ in the original motions.

(6) The motion in each of the components of the disturbed motion is symmetrical about an axis, whatever be the motions of the disturbing atoms, or the original motions relatively disturbed by the atoms.

These properties of a continuous elastic fluid, being assumed to correspond to phænomena of light, give the following theoretical results:—

By (1) and (2) every portion of light consists of *rays*, and the general character of the rays is independent of the circumstances under which the light is generated.

By (3) the whole quantity of the light, the directions of its propagation, and the intensities, refrangibilities, and phases of the component rays, are dependent on the particular circumstances of the disturbance.

When the circumstances of the disturbance are as supposed in (4) (which is the case appropriate to the present inquiry), light may produce new light, which may differ from the original light in intensity, colour, composition, and direction of propagation. This effect I have called a " Transmutation of Rays;" and I beg it may be understood, that in making use of these terms I mean only to express a result deduced from the mathematical theory.

As the directions of propagation of the transmuted rays depend only on the circumstances of the disturbance, the new rays might, consistently with this theory, be propagated only in directions coinciding nearly with the direction of propagation of the original rays.

By (5) it might happen, consistently with the theory, that light is not produced under the circumstances supposed in (4), the values of $\lambda$ for the transmuted rays not falling within the limits proper for visibility; or, that rays for which the values of $\lambda$ do not fall within the limits of visibility give rise to visible rays.

By (6), the transverse motion being always alike in all directions from an axis, the transmuted light can in no case be polarized light.

I proceed now to the consideration of the " Remarks." And in the first place, I am quite ready to admit that Professor Stokes is entitled, by his experimental acquaintance with the class of phænomena under consideration, to set me right in regard to matters of fact, and the terms which appropriately describe them. His remarks have led me to see that I have improperly used the word " epipolic " to signify true internal dispersion in general, of which the phænomenon that the word strictly applies to is only a particular phase. This misapplication of the term, although the sense in which I use it is explained on its first introduction, has not without reason caused some misapprehension of my views. I am not, however, prepared to admit that I have " confused three distinct things," not being able to perceive good reasons for the distinctions that Professor Stokes makes. In the very complete discussion of the various phænomena which is contained in his paper on Change of Refrangibility (Phil. Trans. 1852, part 2. p. 463), I could not make out a distinction between the second and third kinds of phænomena, both being apparently referred to true internal dispersion and change of refrangibility; and with respect to the first, I had reasons, partly theoretical, for referring them to the same head. Change of refrangibility appeared to me the single appropriate idea which embraces all the phænomena, so far as they are not attributable to known causes. I am aware that several experimenters have referred the first kind to known causes and to " subjective illusion." But Sir David Brewster does not admit this explanation, and I did not consider myself at liberty to disregard the opinion on this point of so able and experienced an experimenter. It must also be remembered, that the experimenters referred to had no suspicion of a change of refrangibility, this idea having been first admitted by Professor Stokes after his discovery that invisible rays might give rise to visible. The absence of any such conception is particularly apparent in the experiments of M.

Helmholtz (published in Poggendorff's *Annalen* in 1852), which are proper for decomposing light that might have such an origin, but not proper for discovering that origin. In further explanation of these views, it should be stated that I am led by the theory explained at the beginning of this communication to admit that the new light due to change of refrangibility is not necessarily dispersed in all directions, but may in certain substances deviate but little from the direction of the original light. This, however, is a point which experiment must decide.

The remark that the phænomenon of change of refrangibility " may be set aside in speculating on the sensation of colour," is altogether irrelevant, the question being not about sensation of colour, but as to whether or not a ray of prismatic purity is compounded. From the fact of change of refrangibility by absorbing media, it might fairly be argued that light admits of a resolution different from that effected by the prism. It was therefore an important preliminary to my theory of the composition of colours to establish that this is not the case. Professor Stokes has said that " were a change of colour made out, it would be a point of the utmost importance to consider in reference to any physical theory of light." This importance equally attaches itself to a change of refrangibility, and on this account I endeavoured to explain the fact by reference to the mathematical theory which is stated more at length at the beginning of the present communication. Professor Stokes has shown that he felt the necessity of some theory by speculating largely on the subject in arts. 226 to 236 of his paper, but has not attempted to give a mathematical theory.

The other remarks relate chiefly to composition of colours. Professor Stokes does not admit that a composition of blue and yellow lights can produce green. Referring to an experiment I made with a blue ribbon and a yellow ribbon put in contact and held to the light, which appeared to give a green colour, he says, " the superposition of two coloured glasses or ribbons by no means gives the effect of the mixture of the two colours;" and then adduces an argument which does not apply to the ribbons, because light from one is received through the *interstices* of the other. He makes no allusion to an experiment by Sir John Herschel on which I chiefly relied, in which blue and yellow lines are drawn on paper close together and alternating with each other. Unquestionably in this instance the eye receives a mixture of blue and yellow lights. I have repeated the experiment and find the result to be green. The green is remarkably contrasted with blue, if the blue lines be extended beyond the yellow.

I did not question the fact that " the production of white by a mixture of blue and yellow is by no means confined to

prismatic blue and yellow, but takes place just as well with the colours of bodies," seeing that at the end of the paragraph marked (9) I endeavour, with whatever success, to account for it theoretically.

As little did I question that "in making experiments with the spectrum, in order to neutralize, when possible, a prismatic colour of given intensity by another prismatic colour, so as to produce white, *two* points must be attended to : the place of the second colour in the spectrum must be properly chosen, and the intensity of the light properly regulated;" for I thought I had explained theoretically in the paragraph marked (4), why one of these points must be attended to, and in the paragraph marked (7) why the other must be attended to. There is also another fact which my theory embraces, viz. that when *two given colours* are mixed, the gradations of colour obtained by varying the proportions depend on the *source* of the light. Thus with sunlight the gradations of a mixture of yellow and blue pass from yellow through diluted yellow, white, and diluted blue, to blue; whereas in a mixture of yellow and blue from certain sources, the gradations pass through green.

It is necessary that I should now explain in what sense I used the terms "terrestrial light." The most general composition of a parcel of light may be expressed analytically as follows :—

$$V = m \sin \frac{2\pi}{\lambda} (x - at + c) + m' \sin \frac{2\pi}{\lambda'} (x - at + c') + \&c.,$$

the number of terms being unlimited, and the values of $m$, $m'$, &c., and of $\lambda$, $\lambda'$, &c., either all different, or some equal. The effect of ordinary reflexion and refraction is to alter all the coefficients $m$, $m'$, &c. in a given ratio, and sometimes the phases of the rays, the separate rays retaining their individuality, and the character of the light being in no respect altered. The effect of absorption proper is to alter by quantities depending on the absorbing substance the coefficients $m$, $m'$, &c., reducing some of them to zero while the rays retain their individuality, and to change the phases of the rays, but not to change the values of $\lambda$, $\lambda'$, &c. The effect of true internal dispersion is to alter $\lambda$, $\lambda'$, &c., and completely to change the character of the light. The last effect may sometimes and in some degree accompany the second. Light modified in the second of these ways I have called *terrestrial light,* because the modification takes place by the action of the constituent atoms of some terrestrial substance on sunlight. The term may, however, be extended to other light which is not direct sunlight, or sunlight of the first kind. This statement will explain in what sense I considered the coloured light of substances (which I ascribe to absorption) to be " new

light." It is on the change of the values of *m, m'*, &c. that the difference between the composition of prismatic colours and the composition of the colours of substances depends.

I willingly admit, on Professor Stokes's authority, that "absorption is not always, nor even generally, accompanied by epipolic dispersion." Speaking of "certain absorbing media," I said that "absorption generates in greater or less degree epipolic dispersion," meaning, as already explained, internal dispersion.

I quite concur in referring the natural colours of bodies to absorption. This doctrine I taught long since in optical lectures, and never doubted of its truth.

From the foregoing consideration of Prof. Stokes's Remarks, it appears that there is no material difference in our views except on two points: the real character of the phænomenon presented by certain absorbing substances which has been ascribed to subjective illusion, and the question of fact whether blue and yellow lights in any instance produce a green compound.

Cambridge Observatory,
   December 12, 1856.

## LXVIII. *On the Allotropic Conditions of Sulphur.* By G. MAGNUS [*].

MITSCHERLICH has recently shown[†] that sulphur combined with a fat or an oil, dissolves in sulphur and imparts to it an extremely strong colour, so much so that one part of tallow heated with 3000 parts of sulphur furnishes an intensely red mass. I have repeated these experiments, and have convinced myself that not only the fats, but also a great number of other substances produce a similar action. Besides the fats, the following substances are particularly distinguished for the strong colour they produce:—*stearic acid, paraffine, wax, spermaceti, fossil wax (ozocerite); colophony, mastic, gutta percha,* and *caoutchouc* produce a somewhat less strong colour; and *amber, sugar, starch,* and *cotton* colour still weaker, but even then intensely. The latter only exercise a colouring action when brought into the sulphur heated to 300° C. and well mixed with it.

Some of these substances may have still contained small quantities of fat; of many of them this can scarcely be maintained, but it may be thought that they have acquired a colouring action from having been touched with greasy fingers. In order to meet any objection of this kind, I placed some crystals of white sugar-candy in distilled water, and when the

external layer was dissolved, dried them in porous paper, and without touching them with the fingers, introduced them into the hot sulphur which had been purified by two distillations. Even this coloured the sulphur.

The colour which is imparted to sulphur by sugar, starch, cotton, &c., is indeed more brown than red, but its solution in bisulphide of carbon shows always the pure red colour.

From this it is clear, that other than fatty substances colour sulphur, and the red colour of sulphur cannot serve as an evidence of the presence of a fat. It appears that very various substances so decompose at a higher temperature, that a certain constituent of them unites with the sulphur and forms with it the strongly colouring body. I have, however, not yet been able to effect its separation from the excess of sulphur.

The colouring property of this compound is so great, that not only sulphur which has been touched by the hands is coloured red by the fat which is thereby imparted to it, when heated to 300° C., but also sulphur which has been several times heated to 300° C., and each time poured out, assumes a reddish colour even when it has not been touched by the hands. This probably arises from small particles of dust which during the cooling settle upon it from the air, and enter into the colouring compound at each new melting.

By these experiments I have been led to several facts concerning the allotropic conditions of sulphur. In the establishment of these I had the assistance of M. R. Weber, and to such an extent, that the research may be considered as executed by us in common.

If common yellow sulphur be heated to 350° C., or to that temperature at which, after it has been viscid, it becomes fluid again, and if it be then allowed to cool slowly, it is, as is well known, completely or almost completely soluble in bisulphide of carbon. In order that it may be so, it is good to shake it repeatedly during the cooling, for otherwise that portion on the side of the vessel would cool too rapidly and thereby become insoluble in bisulphide of carbon.

For if sulphur, which has been heated to 300° C., be allowed to cool too rapidly, it is no longer completely soluble in bisulphide of carbon, but leaves, according as the cooling has been more or less rapid, a greater or lesser part undissolved. If the cooling be effected in such a manner that the sulphur be poured in a thin stream into cold water, the well-known white sulphur is obtained. This leaves undissolved, as will be seen from the following Table, from something over thirty to something over forty per cent. of its weight *.

* M. Ch. Saint-Claire Deville states in his treatise *Des modifications du*

But the soluble part of soft sulphur contains still *two* different modifications of sulphur. For if a part of the bisulphide of carbon be distilled off from the solution, and the liquid be then allowed to cool, octahedral sulphur crystallizes out. If this be removed from the liquid and a fresh quantity of bisulphide of carbon be distilled off, so that on cooling some more octahedral sulphur is obtained, there remains, after repeating this operation, a tough mass which can be drawn out in threads. This still contains sulphur. If left to itself the bisulphide of carbon evaporates, and the sulphur separates as a crummy mass. This sulphur was manifestly still more soluble in bisulphide of carbon than octahedral sulphur, for it separated after the latter. Notwithstanding this, after it has been once separated from the bisulphide of carbon, it does not again dissolve in it. If the crummy mass, as is sometimes the case, still contains small crystals of sulphur, these are dissolved, but the crummy sulphur is insoluble even in boiling bisulphide of carbon.

If the soft sulphur, from which the crummy mass has been prepared, has been only *once* heated to 300° C., and if all contact with the fingers has been avoided, the crummy mass has a pure yellow colour. If, on the contrary, the soft sulphur has been several times melted and poured out, it appears more or less red-coloured. On this account I have hitherto designated it as red sulphur. Since it is soluble in bisulphide of carbon, but once separated from this solution does not again dissolve in it, it is distinguished as well from the soluble as from the insoluble sulphur, and must be regarded as a particular allotropic modification. It comports itself in this respect just like silica, which I have also already* noticed with regard to red sulphur. With the exception of the colour, which, as was mentioned, may vary, it exhibits all the properties which have been ascribed to it. If it is melted and slowly cooled, it changes into common soluble sulphur.

The remarkable comportment of the quickly cooled soft sulphur appears to depend on the presence of this modification; for if soft sulphur be allowed to lay so long that it has become hard and brittle, for which, as is known, several days are necessary, it contains much less of this modification than in the fresh, soft condition, as is seen on comparison of the columns 3 and 7 of the following Tables, in which the experiments have been collected.

*soufre sous l'influence de la chaleur et des dissolvants (Ann. de Chim. 3 sér. xlvii. 99), that he found 35 per cent. in it.*

* Phil. Mag. ser. 4. vol. viii. p. 182.

Soft Sulphur extracted with Bisulphide of Carbon immediately on cooling.

| Grammes. | Gave | |
|---|---|---|
| | Insoluble sulphur. | Crummy sulphur. |
| 71 | ......... | 5·32 per cent. |
| 48 | 33·82 per cent. | 2·48 ... |
| 106 | 38·49 ...... | 5·07 ... |
| 100 | 42·00 ... | 3·57 ... |
| 86 | 36·04 ... | 3·91 ... |
| 69 | 46·07 ..... | 2·79 ... |

The same Sulphur perfectly hardened.

| Grammes. | After days. | Gave | |
|---|---|---|---|
| | | Insoluble sulphur. | Crummy sulphur. |
| 71 | 4 | ......... | 0·84 per cent. |
| 33 | 7 | 36·36 per cent. | 1·57 ... |
| 100 | 5 | 37·00 ... | 0·64 ... |
| 79 | 4 | 36·07 ... | 0·77 ... |
| 68 | 4 | 39·70 ... | 0·69 ... |

Whether in the change of the soft sulphur into the hard the crummy sulphur contained in it is changed into insoluble or soluble sulphur, I must for the present leave undecided. It might indeed be believed that this could be settled by comparing the quantity of insoluble sulphur contained in the soft, with the quantity of the same present after it has become hard. But, as is seen from the present experiments, such determinations cannot be executed with sufficient accuracy; partly because the insoluble sulphur from its porous condition easily retains some bisulphide of carbon; but chiefly because we cannot be certain, in spite of all precaution, that the hardened sulphur under investigation reaches the water at exactly the same temperature, and is cooled under exactly the same circumstances,—is thus in the same state as the soft sulphur corresponding to it.

The quantity of crummy sulphur cannot indeed be determined with perfect certainty, but still with sufficient nearness to show that it is present in the soft sulphur in considerably greater quantity than in the hard. On this account I think I may assume, that the flexibility and elasticity of soft sulphur arise from the presence of this modification.

This supposition is rendered more probable by the fact, that even very small quantities of other admixtures change the com-

portment of sulphur in a similar manner; for if sulphur be coloured intensely red or black by one of the above substances, for which only 0·0003 of its weight of paraffine is necessary, and if it be then suddenly cooled, either by pouring it in cold water or on a glass plate, it remains for a long time so soft and greasy that the individual parts of it can be joined together and united into one piece. It may then be drawn out in long thin threads, and retain this adhesive state for several hours, often even for a day,—a comportment which I have also already described in the black sulphur.

But not only the manner of solidifying is altered by this small admixture, but also in the liquid condition does this sulphur exhibit a different comportment to the ordinary sulphur; for it is viscid indeed at a higher temperature, but much less so than common sulphur.

Since the soft and greasy condition of the black sulphur arises from the admixture of so small a quantity of a foreign substance, it is also probable that the flexibility and elasticity of the yellow so-called soft sulphur results from the small quantity of crummy sulphur which it contains.

Such changes in the properties, by the presence of small quantities of foreign substances, are indeed not frequent; yet is an example not wanting, and we only need to be reminded, in this respect, of the change of iron into steel.

The various allotropic conditions of sulphur are hence the following:—

1. *Prismatic* sulphur.
2. *Octahedral* sulphur.

Both soluble in bisulphide of carbon.

3. *Insoluble* sulphur.
4. *Crummy* sulphur, which in the fresh state is soluble in bisulphide of carbon, but once separated from it does not again dissolve in it.

This has been hitherto described by me as soluble and insoluble red sulphur. For if soft sulphur contains even the smallest quantity of colouring substance, it remains constantly with the crummy sulphur, because, like this, it is more soluble in bisulphide of carbon than common sulphur, and hence remains behind with it in the mother liquor, and also, on the complete removal of the bisulphide of carbon, adheres to it in the solid form.

5. The *black* sulphur, which, since it results from an admixture of foreign substances, cannot indeed be considered as an allotropic condition, but as an especial modification of sulphur, because it is distinguished from the various conditions of sulphur not only by the colour, but also by the above-mentioned properties.

## LXIX. *Chemical Notices from Foreign Journals.*
### By E. Atkinson, *Ph.D.*

BERTHELOT* has lately made a very important step in the synthesis of organic compounds. Some time ago he showed that by bringing olefiant gas in contact with the elements of water, alcohol was formed. He has now shown that olefiant gas may be produced by the union of its elements, and has thus completely effected the synthesis of alcohol by means of the simple bodies which constitute it.

When a mixture of bisulphide of carbon and sulphuretted or phosphuretted hydrogen is passed over metallic copper heated to dull redness, there are formed hydrogen, marsh gas, a small quantity of naphthaline, and a considerable quantity of olefiant gas. The marsh gas was separated by means of appropriate solvents, and the olefiant gas absorbed by means of bromine. From the bromine compound the olefiant gas was liberated by a method which Berthelot gives, and its identity established by converting it first into sulphovinate of baryta, and then into the benzoic æther. In the above reaction the copper takes sulphur from the bisulphide of carbon and the sulphuretted hydrogen, and the carbon and hydrogen thus set free unite to form olefiant gas. The naphthaline, hydrogen, and marsh gas may be considered as products of the decomposition of olefiant gas.

It must be noticed that the sulphide of carbon is obtained by the direct union of carbon and sulphur.

In the dry distillation of formiate of baryta, marsh gas, $CH^2$, olefiant gas, $C^4 H^4$, and propylene, $C^6 H^6$, are obtained. Now Berthelot has shown† that the formiates are produced by the direct union of carbonic oxide and the elements of water; and that not only olefiant gas, but also its homologues, may be directly converted into the corresponding alcohols. Here we have another method of the synthesis of alcohol from its elements, for carbonic oxide is formed by the direct union of carbon and oxygen.

Propylene is also formed when carbonic oxide, CO, and marsh gas, $CH^2$, are heated together to dull redness in a closed tube.

In the dry distillation of acetate of soda, olefiant gas, $C^4 H^4$, propylene, $C^6 H^6$, butylene, $C^8 H^8$, and a little amylene, $C^{10} H^{10}$, are obtained. It will be observed that the acetates are prepared from alcohol, which in turn is prepared from olefiant gas. From these various carburetted hydrogens, as was before said, their corresponding alcohols may be obtained. Hence we have the means of building up from one alcohol other and higher alcohols.

* *Comptes Rendus*, July 28, 1856.
† Phil. Mag. July, 1856.

The various carburetted hydrogens obtained in these experiments were combined with bromine. From their respective bromine compounds they may be liberated in a pure state by being heated in a sealed tube in contact with metallic copper, water, and iodide of potassium. If the copper be left out, the hydruret of the carburetted hydrogen is obtained; thus from bromide of olefiant gas, $C^4 H^4 Br^2$, the gas $C^4 H^6$; from bromide of propylene, $C^6 H^6 Br^2$, the gas $C^6 H^8$ are obtained.

From these facts of the relation of carburetted hydrogen to alcohol on the one hand, and of the relation of alcohol to other organic bodies on the other, the complete synthesis of a vast number of organic bodies may be considered as possible.

Wurtz* describes a new series of alcohols. The compound æthers, it is known, are formed by the union of one atom of an acid with one atom of alcohol. Berthelot has found, on the other hand, that glycerine unites with *three* atoms of an acid to form the neutral fatty bodies. There is the same relation between alcohol and glycerine as there is between a monobasic acid and a tribasic acid. If spirit of wine is a monoatomic alcohol, glycerine may be considered as a triatomic alcohol. From these views Wurtz considered that there must be a series of intermediary combinations, whose molecule would be diatomic, and would correspond to the bibasic acids. By combining with *two* equivalents of acid, they would form compounds intermediate between the æthers and the neutral fatty bodies. Wurtz has succeeded in forming by way of synthesis such an alcohol, which he names *glycol*. It is the type of a series, for the method by which it is formed is susceptible of a general application.

By acting on acetate of silver with iodide of æthylene, $C^4 H^4 I^2$, a body is obtained the formation of which is expressed by the following equation :—

$$C^4 H^4 I^2 + 2(Ag O C^4 H^3 O^3) = 2Ag I + C^{12} H^{10} O^8.$$

Iodide of Acetate of silver.     New body.
æthylene.
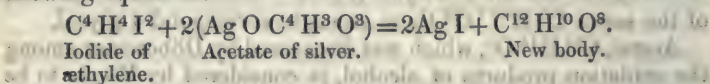
This new body is the acetate of the new alcohol *glycol*. When pure, it is a perfectly colourless, neutral liquid, boiling at 185° C., and distilling unchanged. It is heavier than water, in a large quantity of which it is soluble.

The alcohol itself, glycol, is obtained by acting on acetate of glycol with hydrate of potassa. It is in the pure state a colourless, slightly viscid liquid, having a distinctly saccharine taste. It boils at about 195° C. Its vapour is inflammable; it is soluble in all proportions in water and alcohol.

From the mode of its formation it is evident that the new

* *Comptes Rendus,* July 25 and Sept. 1.

alcohol contains the group æthylene or olefiant gas, $C^4 H^4$. It is derived from the type, two equivalents of water $\left.\begin{array}{c}H^2 \\ H^2\end{array}\right\} O^4$, by the substitution of æthylene for two equivs. of hydrogen; and the acetate of glycol contains two equivs. of the radical acetyle replacing the other two equivalents of hydrogen,—

$$\left.\begin{array}{c}H^2 \\ H^2\end{array}\right\} O^4 \qquad \left.\begin{array}{c}C^4 H^4 \\ H^2\end{array}\right\} O^4 \qquad \left.\begin{array}{c}C^4 H^4 \\ C^4 H^3 O^2 \\ C^4 H^3 O^2\end{array}\right\} O^4.$$

2 equivs. water.      Glycol.      Acetate of glycol.
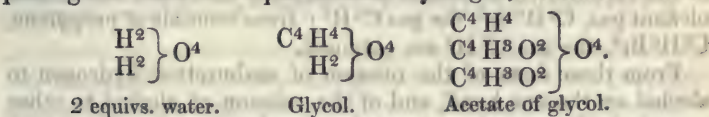
The relations in which alcohol, glycol, and glycerine stand to each other are seen from this table :—

$$\text{Type } \left.\begin{array}{c}H \\ H\end{array}\right\} O^2 \qquad \text{Type } \left.\begin{array}{c}H^2 \\ H^2\end{array}\right\} O^4 \qquad \text{Type } \left.\begin{array}{c}H^3 \\ H^3\end{array}\right\} O^6.$$

Monoatomic alcohol.    Diatomic glycol.    Triatomic glycerine.

$$\left.\begin{array}{c}C^4 H^5 \\ H\end{array}\right\} O^2 \qquad \left.\begin{array}{c}C^4 H^4 \\ H^2\end{array}\right\} O^4 \qquad \left.\begin{array}{c}C^4 H^3 \\ H^3\end{array}\right\} O^6.$$

Alcohol.      Glycol.      Æthylic glycerine.
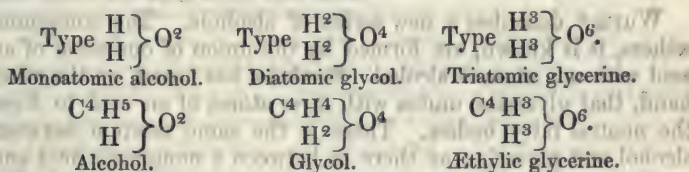
Æthylic glycerine is not as yet known, but Wurtz has obtained a body which is very probably the acetate of æthylic glycerine : ordinary glycerine would be represented by the formula $\left.\begin{array}{c}C^6 H^5 \\ H^3\end{array}\right\} O^6$.

Wurtz has also obtained a benzoate of glycol by acting on benzoate of silver with iodide of æthylene.

By the action of bromide of propylene, $C^6 H^6 Br^2$, on acetate of silver, the acetate of propylic glycol is formed,—

$$C^6 H^6 Br^2 + 2 AgO C^4 H^3 O^3 = 2 Ag Br + \left.\begin{array}{c}C^6 H^6 \\ (C^4 H^3 O^2)^2\end{array}\right\} O^4 ;$$
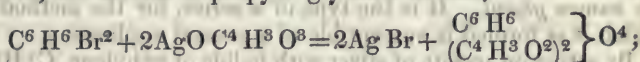
and by the action of bromide of amylene, $C^{10} H^{10} Br^2$, on acetate of silver, the acetate of amylic glycol. Indeed to each alcohol of the series $C^n H^{n+2} O^2$ there seems to correspond a glycol.

Acetal, $C^{12} H^{14} O^4$, which was discovered by Döbereiner among the oxidation products of alcohol, is considered by Wurtz to be a glycol in which two equivalents of hydrogen are replaced by æthyle, $\left.\begin{array}{c}C^4 H^4 \\ C^4 H^5 \\ C^4 H^5\end{array}\right\} O^4$; if this be the case, one or both of the æthylic groups must be capable of being replaced by methyle. Experiment has proved that this is the case. By oxidizing a mixture of wood-spirit and alcohol two compounds were obtained, boiling respectively at 85° and 55°, and having the formulæ—

$$\left.\begin{array}{c}C^4 H^4 \\ C^4 H^5 \\ C^2 H^3\end{array}\right\} O^4 \text{ and } \left.\begin{array}{c}C^4 H^4 \\ C^2 H^3 \\ C^2 H^3\end{array}\right\} O^4.$$
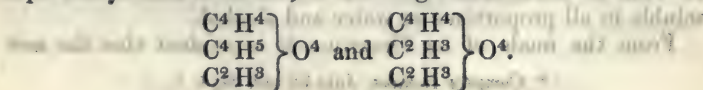
If we represent acetic æther, binacetate of glycol, and triace-tate of glycerine, by the following formulæ,—

$(C^4 H^3 O^3)$, $C^4 H^5 O$, or $\overline{Ae}$ $C^4 H^5 O$, acetate of æthyle,

$(C^4 H^3 O^3)^2$, $C^4 H^4 O^2$, or $\overline{Ae}^2$ $C^4 H^4 O^2$, binacetate of glycol,

$(C^4 H^3 O^3)^3$, $C^6 H^5 O^3$, or $\overline{Ae}^3$ $C^6 H^5 O^3$, trinacetate of glycerine,

we see the application to organic chemistry of the law in mineral chemistry, that the number of equivalents of acid which saturates a base is in relation with the number of equivalents of oxygen which it contains.

In the oil of thyme a solid crystallizable matter is contained, to which the name *thymole* has been given; its formula is $C^{20} H^{14} O^2$, and it is hence homologous with hydrated oxide of phenyle, $C^{12} H^6 O^2$. Its discoverer, Lallemand, has been work-ing out its analogy with this substance, and has arrived at some interesting results[*]. By the action upon it of nitric acid he has obtained a compound, *trinitrothymic acid*, $C^{20} \frac{H^{11}}{3(NO^4)} O^2$, homo-logous with trinitrophenic or picric acid, $C^{12} \frac{H^3}{3(NO^4)} O^2$, and which offers in its physical properties the greatest similarity with this latter acid. By acting upon thymole with chlorine, various sub-stitution products are obtained, one of which, *pentachlorinated thymole*, $C^{20} \frac{Hg}{Cl^5} O^2$, forms hard colourless crystals of the same form as the thymole itself. These fuse without alteration at 98° C., and are decomposed at a temperature of 200° C. into carbon, hydrochloric acid, a new *carburetted hydrogen*, and a solid substance, which, when purified, crystallizes from alcohol in silky needles. It has the formula $C^{14} \frac{H^4}{Cl^4} O^2$, and may be regarded as a derivative of the body $C^{14} H^8 O^2$, isomeric with benzoic alcohol, and the homologue of hydrated oxide of phenyle and of thymole. It might be called *quadrichlorinated benzinole*.

The new carburetted hydrogen is a homologue of marsh gas. It burns with a beautiful white flame, and combines with chlo-rine in equal volumes. Its density at zero is 1·15, and it is not liquefied at 20° below zero. Its formula is $C^4 H^8$; marsh gas, at a condensation of 4 volumes, being $C^2 H^4$. Lallemand names it *biformene*.

The decomposition by which it is obtained may be thus ex-pressed:—

$$2(C^{20}\begin{smallmatrix}H^9\\Cl^5\end{smallmatrix}O^2) = C^{14}\begin{smallmatrix}H^4\\Cl^4\end{smallmatrix}O^2 + 6H\ Cl + C^4\ H^8 + 2CO + C^{20}.$$
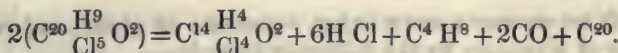
Pentachlorinated   Quadrichlori-          Biformene.
    thymole.       nated benzinole.

From biformene a large number of new bodies may be obtained, and from it, as from propylene, a new series of alcohols may be derived.

By the action of chlorine a heavy ætherial liquid is obtained, boiling at 102° C., and having the formula $C^4\ H^8\ Cl^2$; this, acted upon by potash, gives rise to the formation of a light volatile liquid, of an ætherial alliaceous odour. Its formula is $C^4\ H^7 Cl$, or that of monochlorinated biformene. Bromine and iodine act on biformene in the same manner as chlorine.

When the iodine compound of biformene, $C^4\ H^8\ I^2$, is placed in contact with oxide of silver, a considerable action ensues; a liquid is obtained having an odour like that of wood-spirit, and which gave on analysis numbers corresponding to the formula $C^4\ H^8\ O^2$, which would be *dimethylic alcohol.* By acting on mo-nobrominated biformene, $C^4\ H^7\ Br$, with oxalate of silver, bromide of silver and the oxalate of the new alcohol are obtained. This gives, on treatment with ammonia, oxamide and the new alcohol.

When potash is enclosed with monobrominated biformene in a sealed tube, and exposed to a temperature of 100° C., the æther of the new alcohol is formed. It is a dense gas, burning with a white flame, and is only slightly soluble in alcohol.

Sulphuric acid absorbs about 200 times its volume of bifor-mene, which is not separated on the addition of water. On the addition of carbonate of baryta, a salt is obtained analogous to sulphovinate of baryta, of the formula $2SO^3, C^4\ HgO, BaO$. By appropriate treatment it will give the alcohol $C^4\ H^{10}\ O^2$.

Berthelot[*] publishes a continuation of his researches on the neutral compounds of saccharine matters with the acids. He has produced with mannite and the acids, a series of compounds analogous to those formed by glycerine and the acids. These compounds are produced in general by the direct action of the two substances, in closed tubes, and at a temperature of 200° to 250° C. It is by the same method as that by which, according to Berthelot's experiments, the artificial neutral fatty matters and the compound æthers are formed. These new compounds are separated and purified like the glyceric compounds, by taking advantage of their neutrality, their resistance to alkalies, and their insolubility in æther. They are so similar in appearance to the glyceric series, that they might readily be confounded. They have the same general aspect, a like fusibility, and analogous

* *Annales de Chimie et de Physique*, August 1856.

solubility in the same solvents. But the mannitic compounds possess a greater fixity, a less fluidity, and a stronger tendency to assume the solid form, although these tendencies are not so marked as to excite attention. The compounds are decomposed, under the prolonged influence of the alkalies, with the formation of *mannitane*, or anhydrous mannite, $C^6 H^7 O^5$, which differs from mannite by one equivalent of water, and is transformed into mannite by prolonged contact with water. Mannitane is also formed by heating mannite to 200° C.

With *acetic acid* mannite forms only one compound, a bitter liquid substance; the compound of mannite with *butyric acid* is interesting, inasmuch as both its constituents are entirely derived from the fermentation of sugar, and yet it has all the characters of a neutral fatty matter. With *palmitic*, and with *stearic* acid, mannite forms compounds which are crystalline and resemble palmitine and stearine; when melted, they appear like wax. With *oleic acid*, a neutral, almost colourless substance is obtained, of the consistence of modeller's wax. *Benzoic acid* unites with mannite in two proportions. The first, which consists of one equivalent of acid and one of mannite, is a viscid, almost solid substance; in the other, one equivalent of mannite unites with three of benzoic acid. Both these resemble resins, and are very stable and difficult of decomposition.

Mannite forms with *tartaric acid* an acid compound which is tribasic. It bears the same relation to the benzoic acid compound of mannite which sulphovinic and tartrovinic acids do to benzoic æther. The mannitartaric acid forms with lime a neutral salt containing four equivalents of water, and with magnesia a basic salt containing thirty equivalents of water.

Mannite, heated with *oxalic acid* to 100°, experiences the same decomposition as does glycerine under the same circumstances*; carbonic acid free from carbonic oxide is liberated, and at the same time formic acid is produced, which remains combined with the mannite.

With hydrochloric acid, mannite forms a neutral crystalline compound. By enclosing mannite in a sealed tube along with potash and bromide of æthyle, a compound of mannite and alcohol is formed, analogous to the mixed alcohols of Williamson.

The compounds of mannite with sulphuric acid and with nitric acid are already known. They are also strictly analogous to the compounds formed by glycerine with those acids.

Berthelot and De Luca† have investigated the action of the chloride and bromide of phosphorus on glycerine. The action of

* Phil. Mag. July, 1856.
† *Comptes Rendus*, July 14.

chloride of phosphorus is the same as that of hydrochloric acid; bodies are formed which have already been described by Berthelot. By the action of bromide of phosphorus a large number of bromhydrines are produced; a class of bodies consisting of glycerine which has assimilated hydrobromic acid, and lost the elements of water in various proportions. Of these, the principal are *monobromhydrine, epibromhydrine,* and *dibromhydrine.*

*Dibromhydrine,* $C^6 H^6 Br^2 O^2 = C^6 H^8 O^6 + 2H Br - 4HO$, is a neutral liquid of an ætherial odour similar to that of chlorhydrine. It boils at 219°, and is twice as heavy as water.

When ammonia is pased into its alcoholic solution, bromide of potassium is formed, and the bromide of a new base, *glyceramine* $= N \left. \begin{array}{c} H^3 \\ C^6 H^6 O^4 \end{array} \right\} Br$. The base, separated from the bromide by treatment with caustic potassa, is liquid, very soluble in water and in æther. It is not volatile with the vapour of water, and cannot be distilled without decomposition. It forms with bichloride of platinum a crystalline double salt. This is the first example of a base containing a saccharine matter.

Heated with metallic tin to 140 degrees, dibromhydrine is decomposed with the formation of bromide of tin, and of a peculiar organic compound containing tin; insoluble in water, but soluble in æther, but which could not be obtained in a crystalline form.

Treated with perbromide of phosphorus, dibromhydrine affords compounds in which more equivalents of hydrogen are replaced by bromine.

M. Béchamp* has furnished a most important contribution to physiological chemistry, in effecting a direct proof that the urea in the animal œconomy is derived from albumen or other analogous bodies. He has shown that albumen may be transformed directly into urea by a slow oxidation produced by permanganate of potassa at a temperature of 80°.

M. Dumas, who announced this discovery to the Academy, stated that he had frequently tried to effect the same change. He endeavoured to imitate the process which takes place in the blood by slowly oxidizing the albumen in the presence of an alkali. He had employed bichromate of potash, oxides of mercury and silver, and peroxide of lead with alkalies, but without success.

M. Dumas communicated on the same occasion the results of some experiments of M. Picard, on the presence of urea in the blood and its diffusion through the organism. Long ago Prevost and Dumas had concluded that urea was eliminated by the kidneys,

* *Comptes Rendus,* September 8.

but not produced by them.   M. Picard has completed the proof of this theory.   By the addition to blood of nitrate of mercury he was able to separate every trace of urea from it, and was thus able to compare the proportion of urea in arterial with that in venous blood.   The arterial blood of a dog contained 0·0365 per cent. of urea, while venous blood contained only 0·0186 per cent.   In the human subject, he found that blood which passes through the kidneys would furnish 28 grms. of urea.   Now the quantity of urea contained in the daily secretion of urine amounts to 27 or 28 grms.   The arterial blood which arrives at the kidneys contains more urea than that which passes from it, and the quantity of urea lost in traversing the kidneys corresponds to the quantity found in the urine.   Hence the kidneys do not produce urea, but merely eliminate it.

In the mineral water of Bruckenau in Bavaria, Scheerer[*] has found formic, acetic, propionic, and butyric acids.   The method of detection was as follows :—Very large quantities of the water were evaporated to a small bulk, and the precipitate which formed was filtered off.   The filtrate was distilled with excess of sulphuric acid, and the acid distillate neutralized with baryta.   The mixed baryta-salts were extracted with warm alcohol, which left formiate of baryta undissolved; the presence of this salt was established by the usual confirmatory tests.   To the alcoholic solution of the other baryta-salts sulphate of silver was added, by which the silver-salts were obtained.

From the properties of the silver-salts separated out by successive crystallization, and from determinations of the silver contained in them, evidence of the presence of acetic, propionic, and butyric acids was obtained.   Scheerer considers that these acids are formed by the gradual decomposition of certain constituents of plants dissolved by the rain-water.   He considers that this change is promoted by the alkaline carbonates, which are present in largest quantities in those waters which contain the greatest proportion of these acids, and also by the presence of the oxides of iron and manganese.   The occurrence of organic acids in mineral waters is more frequent than is generally thought : Lehmann has recently found in the Marienbad water, formic, acetic, succinic, and even oleic and stearic acids.

M. Belhomme[†] has ascertained the presence of a colouring matter in *Monarda didyma,* which from all its properties is evidently carmine.   It resides principally in the corolla; and as it is contained in tolerable quantity, it may be advantageously extracted and had at a cheap rate.   Samples of silks dyed with it have given satisfactory results.

* Liebig's *Annalen,* Sept. 1856.      † *Comptes Rendus,* Aug. 18, 1856.

## LXX. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

#### [Continued from p. 481.]

April 24, 1856.—The Lord Wrottesley, President, in the Chair.
THE following communications were read :—

" Elements of a Mathematical Theory of Elasticity." By Professor William Thomson, F.R.S.

This paper consists of two parts: Part I. on Stresses and Strains; Part II. on the Mechanical Conditions of Relation between Stresses and Strains experienced by an Elastic Solid.

Part I.—The terms *Stress* and *Strain* are used in accordance with the valuable definitions by which they were first distinctively introduced into the Theory of Elasticity by Mr. Rankine\*; with only this deviation; that instead of defining a stress as the reactive force exerted by an elastic body when in a condition of strain, the author of the present paper defines stress as " a definite external application of force to a body."

Various well-known theorems regarding the geometrical relations of the displacements among the parts of a body in a state of strain, and the geometrical representation of stresses and strains are enunciated, and briefly demonstrated, for the sake of convenience. A mode of expressing in absolute measure the magnitude of a stress or a strain, which the author believes to be new, is laid down nearly in the following terms. *The amount of work done by a stress applied to a body of unit volume, while acquiring a strain of the same type as the stress, is measured by the product of the magnitude of the stress into the magnitude of the strain.*

When a stress and a strain are of the same type, they are said to be concurrent; or, if directly opposed, they are said to be negatively concurrent. When a stress and a strain are of any different types, the degree of their concurrence, or simply " their concurrence," is measured by *the work done by the stress applied to a body of unit volume acquiring the strain, divided by the product of the magnitude of the stress into the magnitude of the strain.* The measure of perfect concurrence is therefore $+1$, and that of perfect opposition $-1$. When work is neither spent nor gained in the application of a certain stress to a body while acquiring a certain strain, that stress and that strain, or any stresses or strains of the same types respectively, are said to be orthogonal to one another. The measure of their concurrence is zero.

A system of stress or strain coordinates involving symmetrically six independent variables, perfectly analogous to the system of triple coordinates for specifying the position of a point in space, is laid down. The concurrence of a stress or strain with six orthogonal types of reference being denoted by $l, m, n, \lambda, \mu, \nu$, it is demonstrated that

$$l^2 + m^2 + n^2 + \lambda^2 \quad \mu^2 + \nu^2 = 1,$$

and it is proved that if $\cos \theta$ denote the mutual concurrence between two stress or strain types, whose concurrences with six orthogonal

\* " On Axes of Elasticity and Crystalline Forms," Phil. Mag. vol. xi. p. 301.

types of reference are respectively $(l, m, n, \lambda, \mu, \nu)$ and $(l', m', n', \lambda', \mu', \nu')$, we have

$$\cos \theta = ll' + mm' + nn' + \lambda\lambda' + \mu\mu' + \nu\nu'.$$

The treatment of the subject in the text of the paper is quite abstract, but along with it a series of examples are given, illustrating the statements by applications to familiar types of stresses and strains.

Part II. commences with an interpretation of the Differential Equation of the potential energy of Elasticity of a solid, in terms of the mode of specification of stresses and strains laid down in Part I. The Quadratic Function expressing the potential energy of an elastic solid when strained to an infinitely small amount, is next considered ; and its simplest possible form, that of six squares with coefficients, is interpreted. Hence it is proved that *an infinite number of systems of six types of strains or stresses exist in any given elastic solid, such that if a strain of any one of those types be impressed on the body, the elastic reaction is balanced by a stress orthogonal to the five others of the same system.*

It is next shown that there is necessarily one, and in general only one, such system of six types of strain for an elastic solid which are all mutually orthogonal ; and the types belonging to this system are called the Six Principal Strain Types of the body.

The characteristic of a Principal Strain Type is, that the *stress* required to keep a body in a state of strain of such a type, *is of the same type as the strain*. The six Principal Elasticities of a body are the six coefficients by which strains of the six Principal Types must be multiplied to find the stress required to maintain them.

In conclusion, reasons are given for believing that natural crystals may exist for which there are six unequal Principal Elasticities, and consequently six different, and only six different, Principal Strain-types.

A corollary regarding the property which certain liquids and crystals possess of causing a rotation in the plane of polarization of light passing through them, and Faraday's optical property of transparent bodies under magnetic force, is inferred, and is more fully considered in a subsequent communication to the Royal Society.

"On the Construction of the Imperial Standard Pound, and its Copies of Platinum ; and on the comparison of the Imperial Standard Pound with the Kilogramme des Archives." By W. H. Miller, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge.— Part I.

The Commissioners appointed in 1838 to consider the steps to be taken for the restoration of the standards of weight and measure, to replace those which were destroyed by the burning of the Houses of Parliament, found provisions for the restoration of the lost standards prescribed to them by Sections 3 and 5 of the Act 5th George IV., whereby it is directed that, in case of the loss of the standards, the yard shall be restored by taking the length which shall bear a certain relation to the length of the pendulum, vibrating seconds of mean time, in a vacuum, at the level of the sea ; and that the pound shall be restored by taking the weight which bears a certain proportion to

the weight of a cubic inch of water weighed in a certain manner. The Commissioners, however, in their Report dated December 21, 1841, decline to recommend the adoption of these provisions for the following reasons: "Since the passing of the said Act it has been ascertained that several elements of reduction of the pendulum experiment therein referred to are doubtful or erroneous. It is evident, therefore, that the course prescribed by the Act would not necessarily reproduce the length of the original yard. It appears also that the determination of the weight of a cubic inch of water is yet doubtful (the greatest difference between the best English, French, Austrian, Swedish and Russian determinations being about $\frac{1}{1200}$ of the whole weight, whereas the mere operation of weighing may be performed to the accuracy of $\frac{1}{1,000,000}$ of the whole weight). Several measures, however, exist, which were most carefully compared with the former standard yard; and several metallic weights exist which were most accurately compared with the former standard pound; and by the use of these the values of the original standards can be respectively restored without sensible error. And we are fully persuaded that, with reasonable precautions, it will always be possible to provide for the accurate restoration of standards by means of material copies which have been carefully compared with them, more securely than by experiments referring to natural constants."

At the end of the Travaux de la Commission pour fixer les Mesures et les Poids de l'Empire de Russie, Professor Kupffer has collected the results of observations made in France, England, Sweden, Austria and Russia for finding the weight of a given volume of water. The resulting values of the weight of an English cubic inch of water in a vacuum at 62° Fahr., expressed in doli, of which 22504·86 make a kilogramme, are as follows:—

| | |
|---|---|
| French observations | 368·365 |
| English observations | 368·542 |
| Swedish observations | 368·474 |
| Austrian observations | 368·237 |
| Russian observations | 368·361 |

Assuming the Russian observations to be the best, as they probably are, it will be seen that a troy pound deduced according to the method prescribed by the Act, would be 2·829 grains too heavy; while, if the Austrian observations had been accepted as the best, the troy pound would have been 4·707 grains too heavy. On the other hand, it was possible to recover the weight of the lost standard in air to within a fraction of 0·001 grain, by means of the troy pounds which had been compared with it, and could be easily brought together for recomparison. Seeing, then, that the error of one of these two methods of restoring the lost standard, is at least 2829 times as large as the error of the other method, the Committee could not hesitate to recommend the adoption of the latter.

A Committee was appointed by a Treasury-Minute of June 20, 1843, to carry out the recommendations contained in the Report referred to above. The evidence for ascertaining the weight of the lost standard, placed at the service of this Committee, consisted of the

following weights :—The brass troy pounds of the Exchequer Office ; the brass troy pounds from the cities of London, Edinburgh and Dublin ; the platinum troy pound and the two brass troy pounds then in the possession of Professor Schumacher ; the platinum troy pound of the Royal Society ; the troy pound used by the late Mr. Robinson of Devonshire Street, purchased by the Committee ; four troy pounds made in 1758, two of which were formerly in the possession of Mr. Bingley of the Royal Mint, one the property of Messrs. Vandome and Titford, and one the property of the Bank of England.

The troy pounds of the Exchequer, and of the cities of London, Edinburgh and Dublin had been compared with the lost standard by Captain Kater in 1824. The three troy pounds in the custody of Professor Schumacher, and the troy pound of the Royal Society, were compared with the lost standard with extraordinary care in 1829 by the late Captain v. Nehus. The troy pounds bearing the date 1758 were constructed, along with the lost standard, by Mr. Harris, Assay Master of the Mint. These were referred to at the suggestion of Professor Schumacher, in the hope of arriving at a knowledge of the volume of the lost standard, which, unfortunately, had never been determined by weighing it in water. For, as long as the volume of the lost standard remains unknown, the weight of the air displaced by it, and, consequently, its absolute weight, is uncertain within limits far exceeding the errors of weighing.

Let U denote the lost standard ; Ex, L, Ed, D, RM the troy pounds of the Exchequer, the cities of London, Edinburgh, Dublin, and the Royal Mint, respectively ; Sb, K two brass troy pounds, Sp a platinum troy pound, all in the custody of Professor Schumacher ; RS the platinum troy pound of the Royal Society. Let $\Delta$ prefixed to the symbol by which any weight is designated denote the ratio of the density of the weight at the freezing-point to the maximum density of water ; $t$ the temperature of the air in degrees of Fahrenheit's scale ; $b$ the height of the mercury in the barometer in inches reduced to the freezing-point. The symbol $\rightleftharpoons$ placed between the symbols of two weights will be used to denote that they appear to be equal when weighed in air. The two weights in this case will not be equal unless their volumes are equal. When the weighings have been made in air of given density, or have been reduced to what they would have been in air of given density ; or when, the volumes of the weights and the temperatures and pressures of the air being unknown, we are compelled to assume that their volumes are equal, the symbol $=$ may be substituted for $\rightleftharpoons$.

By the observations of Captain Kater (Philosophical Transactions, 1826),—

$$Ex = U + 0\cdot0010$$
$$L = U + 0\cdot0005$$
$$Ed = U + 0\cdot0015$$
$$D = U + 0\cdot0022$$
$$RM = U + 0\cdot0021$$

By the observations of Captain v. Nehus in 1829—

| No. of obs. | | $b.$ | $t.$ |
|---|---|---|---|
| 300 | Sp $\simeq$ U $-0.00857$ | 29.722 | 65.62 |
| 140 | RS $\simeq$ U $-0.00205$ | 29.806 | 65.73 |
| 60 | Sb $\simeq$ U $-0.01034$ | 29.965 | 64.50 |
| 92 | K $\simeq$ U $+0.03389$ | 29.646 | 65.09 |
| 16 | RM $\simeq$ U $+0.00887$ | 29.679 | 65.91 |

$$10-\log \Delta \text{ Sp}=8.67392, \ 10-\log \Delta \text{ RS}=8.67392,$$
$$10-\log \Delta \text{ Sb}=9.08471, \ 10-\log \Delta \text{ K}=9.09724.$$

These weights were afterwards compared with each other with a balance of extreme delicacy procured from Mr. Barrow. In its construction it nearly resembled the balances of the late Mr. T. C. Robinson. The beam is made sufficiently strong to carry a kilogramme in each pan. Instead of having an index pointing downwards, as is usual in balances of this description, a thin slip of ivory is affixed to one end of the beam, a little more than half an inch long, divided into spaces of about 0.01 inch each. This scale is viewed through a compound microscope having a single horizontal wire in the focus of the eye-piece. A screen was interposed between the observer and the front of the balance-case, having a very small opening opposite to the eye-piece of the microscope.

In making a large number of comparisons, the weights compared are exposed to the risk of being injured by wear. In order to obviate this danger, two light pans were used of very nearly equal weight, each of which has a loop of wire forming an arch, the ends of which are attached to the pan at opposite extremities of a diameter of the pan. To the upper point of the loop of wire is affixed an iron hook. Each pan is suspended by a wire of suitable length bent into a hook at either end, from the ring attached to the agate plane resting on the knife-edge at either end of the balance.

Calling the weights of the pans X and Y, and the weights to be compared P and Q, P was placed in X and Q in Y, and P + X compared with Q + Y $n$ times; then P was placed in Y and Q in X, and P + Y compared $n$ times with Q + X. The weights were thus exposed to the wear of two ordinary comparisons only in the course of $2n$ comparisons. The mean of the $2n$ comparisons gives the difference between P and Q unaffected by the very small, but unknown difference between X and Y. This contrivance was found to be especially useful when either of the weights to be compared consisted of several parts.

In using the method of double-weighing, the counterpoise was placed in the left-hand pan of the balance, and the detached pan X containing the weight P, and the detached pan Y containing the weight Q, were alternately suspended from the right-hand end of the beam, and the positions of equilibrium deduced in each case from the extreme positions of the beam at the beginning of each of three consecutive oscillations (usually twenty times). The weights were then interchanged, and the pan Y containing the weight P, and the pan X containing the weight Q, suspended alternately from the right-hand end of the beam the same number of times.

In weighing by Gauss's method, in which the two weights to be compared are suspended from the right and left-hand ends of

the beam respectively, and are then interchanged, it was desirable to be able to transfer the pans and the weights contained in them from one end of the beam to the other, without opening the doors of the balance-case, and thus avoid sudden changes of temperature of air in the balance-case, and consequent production of currents of air. In order to effect this, a slender brass tube 38 inches long was made to pass freely through two holes in the ends of the balance-case, which is nearly 23 inches long, near the top of the case and half-way between the balance and the front of the case. To the middle of the tube is attached a depending loop of wire. Suppose that by sliding the tube the loop is brought near to the right-hand end of the beam, and the pan with a weight in it transferred from the end of the beam to the wire loop by a brass rod having a hook at the end, which is inserted through a hole in the right-hand end of the balance-case. By sliding the tube in the opposite direction, the loop with the pan and weight suspended from it, is brought near to the left-hand end of the beam, to which it is transferred by a brass rod having a hook at the end, passing through a hole in the left-hand end of the balance-case. A similar tube half-way between the balance and the back of the case, serves to transfer the other pan and weight from one end of the beam to the other. In this manner any number of comparisons may be made without opening the balance-case, except in the middle of the series, for the purpose of changing the pans.

A sufficient number of preliminary comparisons of Sp, RS, Sb, K, Ex, L, Ed having been made in 1844, the results were reduced, when the material of one weight was platinum and that of the other brass, to what they would have been in air ($t=65\cdot66$, $b=29\cdot75$), or, of the mean density of the air during the comparisons of Sp and RS with U in 1829. Using U, Sp, RS, &c. to denote the apparent weights of U, RS, &c. in air ($t=65\cdot66$, $b=29\cdot75$), it was found that—

| In 1829. | In 1844. |
|---|---|
| gr. | gr. |
| RS = Sp + 0·0051 | RS = Sp + 0·0057 |
| Sp = Sb + 0·0022 | Sp = Sb + 0·0030 |
| RS = Sb + 0·0073 | RS = Sb + 0·0032 |
| K = Sp + 0·0420 | K = Sp + 0·0362 |
| K = RS + 0·0369 | K = RS + 0·0304 |
| K = Sb + 0·0442 | K = Sb + 0·0317 |

In the interval between 1829 and 1844, the difference between the two platinum troy pounds Sp and RS had undergone no very sensible relative change. If, as appears probable, Sp and RS have undergone no sensible absolute change, Sb has gained 0·0046 grain, and K has lost 0·0061 grain. On the same supposition it appears that—

| In 1824. | In 1844. | Increase of | Interval in years. |
|---|---|---|---|
| gr. | gr. | gr. | |
| Ex − U = + 0·0010 | Ex − U = + 0·0099 | Ex 0·0089 | 20 |
| L − U = + 0·0005 | L − U = + 0·0151 | L 0·0146 | 20 |
| Ed − U = − 0·0015 | Ed − U = + 0·0206 | Ed 0·0221 | 20 |
| D − U = + 0·0022 | D − U = + 0·0248 | D 0·0226 | 20 |
| RM − U = + 0·0021 | (1829) RM − U = + 0·0089 | RM 0·0068 | 5 |

With the single exception of K, all the brass weights have become heavier since they were compared with U, in consequence probably of the oxidation of their surfaces, while U, which was made in 1758, was protected from further change by the coat of oxide already formed. One of these weights, Sb, appeared to have been protected by gilding, though imperfectly, since parts of its surface were slightly tarnished. Ex and L were brighter than Ed and D. K, though it had become lighter, was much tarnished. The discordances presented by the different weighings of K appear to have greatly perplexed both Professor Schumacher and Captain Kater, and were probably the cause of the numerous and accurate comparisons of the several troy pounds placed at the disposal of the Committee with the lost standard, on which alone depends the possibility of restoring it with sufficient accuracy. Previous to the comparison of K in 1844, a small fragment of wood, like a grain of coarse sawdust, was found adhering so firmly to its under surface, that it was detached with some difficulty. It appears probable that the changes of the weight of K were caused by this bit of wood being weighed with it after the first comparison of K by Captain Kater, and by the gradual oxidation of the surface of K. The discrepancies presented by the weighings of the brass troy pounds at different times, due to the effect of oxidation or other causes, are so large, that I resolved, with the consent of the Astronomer Royal, to rest for the evidence of the weight of the lost standard entirely on the 300 comparisons of Sp and the 140 comparisons of RS with U.

If we consider the discordances presented by the weighings of the brass troy pounds simply as errors of observation, without paying any regard to their probable causes, the resulting value of U will not be very different from that given by the platinum troy pounds alone.

By the observations of 1824 and 1829,

|  | | gr. | weight. |
|---|---|---|---|
| U=Sp | | $+0\cdot0081$ | 30 |
| U=RS | | $+0\cdot0030$ | 14 |
| U=Sb | | $+0\cdot0103$ | 6 |
| U=K | | $-0\cdot0339$ | 9 |
| U=$\frac{1}{4}$(Ex+L+Ed+D) | | $-0\cdot0022$ | 6 |

By the observations of 1844,

|  | | | gr. |
|---|---|---|---|
| RS | =Sp | | $+0\cdot0057$ |
| Sb | =Sp | | $+0\cdot0030$ |
| K | =Sp | | $+0\cdot0363$ |
| Ex+L+Ed+D | =2(Sb+K) | | $+0\cdot0260$ |

Whence, supposing the errors of weighing in 1844 to be insensible, compared with the discordances of the brass troy pounds,

|  | | gr. | weight. |
|---|---|---|---|
| (1) | U=Sp$+0\cdot0081$ | | 30 |
| (2) | U=Sp$+0\cdot0087$ | | 14 |
| (3) | U=Sp$+0\cdot0133$ | | 6 |
| (4) | U=Sp$+0\cdot0024$ | | 9 |
| (5) | U=Sp$+0\cdot0261$ | | 6 |

The mean of all the equations gives U=Sp$+0\cdot0096$ grain.

Excluding the last, which depends upon the weighings in 1824, U=Sp+0·0079 grain.

Excluding all except the results of the comparisons of U with the two platinum troy pounds, U=Sp+0·0083 grain.

The temperatures were determined by means of three thermometers by Bunten, having centesimal scales etched upon the tube, and two thermometers having arbitrary scales traced upon the tubes with a diamond point. The zero-points of these were determined at distant intervals. They were often compared with each other, and, lastly, with an excellent standard thermometer constructed at Kew under the directions of Mr. Welsh, in order to form tables of the errors at any point of their scales, and to determine the position of their zeros at any given time. The barometer employed was a portable cistern barometer by Ernst of Paris, the scale of which was divided into millimetres. It was compared first with the standard barometer of the Paris Observatory, and afterwards with a standard barometer, having a tube of very large bore, belonging to the Taylor Library of Sidney Sussex College, Cambridge.

According to Ritter (Mémoires de la Société de Physique de Genève, t. iii. p. 361), the observations of Regnault show that in Paris, lat. 48° 50′ 14″, 60 metres above the mean level of the sea, a litre of dry atmospheric air, containing the average amount, 0·0004 of its volume, of carbonic acid, the density of which is 1·529 of that of atmospheric air at 0° Cent., under the pressure of 760 mm. of mercury at 0° Cent., weighs 1·2934963 gramme. If G be taken to denote the force of gravity at the mean level of the sea in lat. 45°, the force of gravity in lat. λ, at the mean level of the sea, $=$G$(1-0·0025659\cos 2\lambda)$ (Baily, Mem. Ast. Soc. vol. vii. p. 94). The force of gravity in a given latitude at a place on the surface of the earth at the height $z$ above the mean level of the sea $=\left\{1-\left(2-\frac{3}{2}\frac{\rho'}{\rho}\right)\frac{z}{r}\right\}\times$ force of gravity at the level of the sea in the same latitude, where $r$ is the radius of the earth, $\rho$ its mean density, and $\rho'$ the density of that part of the earth which is above the mean level of the sea (Poisson, Traité de Mécanique, t. ii. p. 629).

According to Regnault, the expansion of air under constant pressure from 0° to 100 Cent., is 0·36706 of its volume at 0° Cent.; also at 50° Cent., the mercurial thermometer is about 0°·2 in advance of the air thermometer (Mémoires de l'Institut, t. xxi. p. 91. p. 238, Annales de Chimie, 3 série, t. v. p. 99). Hence, density air at 0° : density air at $t=1+0·003656t$. The density of the vapour of water is 0·622 of that of air. Hence, if $t$ be the temperature of the air in centesimal degrees, $b$ its barometric pressure, $v$ the pressure of vapour, both in millimetres of mercury at 0° Cent., the weight in grammes of a litre of air at a place on the surface of the earth at a height $z$ above the mean level of the sea in lat. λ, will be

$$\frac{1·2930693}{1+0·003656t}\frac{b-0·378v}{760}\left(1-1·32\frac{z}{r}\right)(1-0·0025659\cos 2\lambda).$$

Regnault finds that in rooms not heated artificially, the pressure of vapour is two-thirds of the maximum pressure corresponding to the

temperature (Memorie della Società Italiana della Scienze in Modena, t. xxv. p. 1).

The weight of air used in reducing the weighings was calculated from the above expression.

The mean rate of expansion of brass, for 1° Cent., from 0° Cent. to 100° Cent., usually assumed 0·0000187 of its length at 0° Cent., is considerably larger than the rate of expansion at ordinary atmospheric temperatures, according to the observations of Mr. Sheepshanks, who found that at about 17° Cent. the coefficient of the linear expansion of brass $=0·00001722$ for 1° Cent. This value of the expansion has been accordingly adopted.

The linear expansion of platinum is assumed to be 0·00000900 for 1° Cent., as given by Schumacher in his first table (Phil. Trans. 1836). The expansion of water is calculated from a mean of the experiments of Despretz, I. Pierre and Kopp, corrected for the error of the assumed expansion of mercury by Regnault's observations, and assuming the temperature of maximum density to be 3°·945 Cent., in accordance with the result obtained by Messrs. Playfair and Joule. The logarithms of the expansion to 7 places considered as integers, are given with sufficient accuracy, between 4° Cent. and 25° Cent., by $32·72(t-3·945)^2-0·215(t-3·945)^3$.

Though it appears that only two of the nine weights with which U was compared in 1826 and 1829 are in a state of unexceptionable preservation, and that the number of trustworthy comparisons is reduced from 669 to 440, these are amply sufficient for the purpose of ascertaining the weight of U in air ($t=65°·66$ Fahr., $b=29·75$ inches). But in order to find the absolute weight of U, or indeed its apparent weight in air of a density different from that which it has when $t=65°·66$ Fahr., $b=29·75$ inches, a knowledge of the volume of the lost standard is requisite. An indirect way of arriving at it was suggested by Professor Schumacher, by an examination of certain Parliamentary Reports, presented May 26, 1758, April 11, 1759, March 2, 1824. It appears from the first of these, that Mr. Harris, then Assay Master of the Mint, presented to the Committee three troy pounds made under his direction, one of which was the lost Imperial standard; and from the third, that one of the two remaining pounds came into the possession of Mr. Vandome, and the second into the possession of Mr. Bingley of the Mint. Professor Schumacher then observes that we can still either determine, with the highest degree of probability, the density of the lost Imperial standard, or know with certainty that all hope to arrive at this knowledge is lost. It will be only requisite to ascertain with the greatest care the densities of both pounds, the one in the possession of Mr. Bingley, the other in the possession of Mr. Vandome. If the density of both is found the *same*, we might from that circumstance draw the highly probable conclusion, that the three single pounds of Mr. Harris, according to my hypothesis, were really made of the same identical metal; and the density of the two remaining pounds might with safety be considered as that of the lost standard. If, on the contrary, the two remaining pounds prove to be of *different* den-

sities, the hypothesis that all three were made of the same metal is
evidently erroneous ; and nothing can be inferred from the density of
either of the two remaining.

Mr. Vandome readily consented to allow his troy pound to be
experimented upon by the Committee. Denoting this weight by
the letter V, by weighing in air and in water it was found that
$\Delta V = 8\cdot15084$, and that it was about $0\cdot309$ grain lighter than U.

Mr. Bingley had in his possession two troy pounds, both dated
1758. One of these, O, said to be the original weight from which
the standard was made for the House of Commons in 1758, has
since been purchased by the Committee ; the other, M, has been pre-
sented to the Mint by Mr. Bingley. As Mr. Bingley was unwilling
to permit either of these weights to be weighed in water, Messrs.
Troughton and Simms were commissioned to construct an instru-
ment on the principle of the Stereometer invented by M. Say for the
purpose of finding the density of gunpowder (Ann. de Chimie, 1797,
t. xxiii. p. 1), but with some improvements which I had described
in the Philosophical Magazine for July and December, 1834, vol. v.
p. 203. Let $v$ prefixed to the symbol of any weight denote the
volume of that weight at $0°$ Cent., the unit of volume being the
volume of a grain of water at its maximum density. Then, by
means of the Stereometer, it was found that $v\mathrm{V} - v\mathrm{O} = 22\cdot68$,
$v\mathrm{V} - v\mathrm{M} = 17\cdot38$. These differences show that the volume of the lost
standard cannot be inferred with any high degree of probability from
a comparison of the remaining pounds. The only resource now
remaining was indicated by Professor Schumacher's remarks on the
figure of the lost standard :—" As soon as the Imperial standard
troy pound was brought to Somerset House, Captain Nehus's first
care was to make an accurate drawing of its shape and marks,
measuring all its dimensions with the greatest care. The annexed
drawing represents this pound in its actual dimensions ; and is now,
since the original has been destroyed by the calamitous fire that con-
sumed the two Houses of Parliament in 1834, the only thing remain-
ing which can preserve an idea of it." By a comparison of the figure
of U in the Philosophical Transactions for 1836, with a profile of V
traced mechanically, the axis and the extreme diameter of the knob
and cylindrical portion of U, appeared to be a very little greater than
the corresponding dimensions of V. On comparing the profiles of
U and V, it did not seem possible to suppose that the volume of U
was less than that of V. But the volume of O, as well as that of M,
being less than that of V, it appeared that of the three weights V,
O, M, V approximated most nearly to U in volume. As the exist-
ing data were utterly insufficient to determine how much, if at all,
U exceeded V in volume, it appeared safest to assume the volumes of
U and V to have been equal. This course was also recommended
by Professor Schumacher.

It was afterwards found that O was $0\cdot144$ grain lighter than U,
$\Delta O = 8\cdot4004$ ; and that M was $0\cdot047$ grain lighter than U,
$\Delta M = 8\cdot3491$.

In a letter from William Miller, Esq., of the Bank of England,

dated August 22, 1855, I was apprised of the existence of a fourth troy pound of 1758. This weight was 0·249 grain heavier than U ; its density =8·3175.

If U, the lost standard, be supposed to have the same density as V, the comparisons of Sp and RS with U by Captain v. Nehus in 1829, give,

$$Sp = U - 0.52959$$
$$RS = U - 0.52444.$$

The Commissioners for the Restoration of the Standards of Weight and Measure, in their Report, dated December 21, 1841, recommended that the avoirdupois pound of 7000 grains be adopted instead of the troy pound of 5760 grains, as the New Parliamentary Standard of Weight, and that the new standard and four copies of it be constructed of platinum.

In accordance with this recommendation, five weights were made by Mr. Barrow, a little in excess of 7000 grains, of platinum prepared by Messrs. Johnson and Cock. The form of these pounds is that of a cylinder, nearly 1·32 inch in height and 1·15 inch in diameter, with a groove round it, the middle of which is about 0·34 inch below the top of the cylinder, for insertion of the prongs of a forked lifter of ivory. They are marked PS 1844 1 lb. ; PC No. 1 1844 1 lb. ; PC No. 2 1844 1 lb.; PC No. 3 1844 1 lb.; PC No. 4 1844 1 lb., respectively.

The weights of 7000 grains might have been derived from that of 5760 grains, by the use of either a decimal or a binary system of weights. In either case, however, the number of weights to be compared with one another and with the weights of 7000 and 5760 grains would have been large, and the errors of their comparisons among themselves might, by their accumulation, sensibly affect the resulting weight of 7000 grains. Also, the repeated comparison of weights made up of the sum of several others, was a very troublesome process, previous to the use of the detached pans, already described, which had not been thought of when the weights were ordered.

These two evils were in a great measure avoided by the use of a platinum weight T of about 5760 grains, or more correctly very nearly equal to Sp or RS, and of the following auxiliary weights, also of platinum, and all constructed by Mr. Barrow: A, B, C, D each of 1240 grains ; F of 800 grains ; G of 440 grains ; H of 360 grains ; K, L, M, N each of 80 grains ; R, S each of 40 grains, nearly. The numbers of the weights of each denomination, and their values, are given by the quotients and divisors obtained in the conversion of $\frac{7000}{5760}$ into a continued fraction. The errors of these weights are found by the following comparisons:—Sp and RS with T ; T with A+B+C+D+F ; each of the weights A, B, C, D with F+G ; F with G+H ; G with each of the weights H+K, H+L, H+M, H+N ; H with K+L+M+N+R and K+L+M+N+S ; each of the weights K, L, M, N with R+S.

Sp and RS, instead of being true troy pounds, and, consequently, equal to U in a vacuum, had been adjusted so as to appear nearly as heavy as U when weighed in air of ordinary density, and are therefore lighter than U by about 0·53 grain, the weight of the air contained in the space equal to the difference between the volume of

U and that of Sp or RS. A space equal to the difference between the volume of 7000 grains of metal of the assumed density of U, and 7000 grains of platinum, contains about 0·645 grain of air. Calling this Q, PS may be compared with each of the weights $T+A+Q$, $T+B+Q$, $T+C+Q$, $T+D+Q$. In order to determine Q with the greatest precision, Mr. Barrow supplied ten weights Q of about 0·645 grain each, so accurately adjusted that no appreciable difference could be detected between them; a weight V of 6·451 grains, and a weight W of 12·901 grains, all of platinum. Then Y and Z being two platinum weights of 20 grains each, the following comparisons became possible :—each of the weights R and S with $Y+Z$ ; each of the weights Y and Z with $W+V+$ each of the weights Q in turn ; W with $V+$ sum of ten weights Q ; V with the sum of the ten weights Q. In comparing PS with each of the weights $T+A+Q$, $T+B+Q$, $T+C+Q$, $T+D+Q$, the weight Q was changed at the end of every four comparisons, and thus each of the ten weights Q used in turn in a series of forty comparisons.

By numerous weighings in air and in water the densities of the several weights were found to be as follows :—

$$
\begin{aligned}
&\text{T} \ldots\ldots\ldots\ldots \quad 21 \cdot 1661\\
&\text{PS} \ldots\ldots\ldots \quad 21 \cdot 1572\\
&\text{PC No. 1} \ldots \quad 21 \cdot 1671\\
&\text{PC No. 2} \ldots \quad 21 \cdot 1640\\
&\text{PC No. 3} \ldots \quad 21 \cdot 1615\\
&\text{PC No. 4} \ldots \quad 21 \cdot 1556
\end{aligned}
$$

By 286 comparisons of T with Sp and 122 comparisons of T with RS, assuming the density of U to have been the same as that of V, $T=5759 \cdot 47141$ grains, of which U contained 5760. By numerous comparisons of the auxiliary weights with each other and with T, $A=1239 \cdot 88621$, $B=1239 \cdot 88604$, $C=1239 \cdot 88596$, $D=1239 \cdot 88579$, $Q=0 \cdot 64509$.

By 80 comparisons of PS with each of the weights $T+Q+A$, $T+Q+B$, $T+Q+C$, and 100 of PS with $T+Q+D$,

| | | gr. | t. | b. |
|---|---|---|---|---|
| PS $\rightleftharpoons$ | $T+Q+A-$ | $0 \cdot 002936$ | $19 \cdot 47$ | $758 \cdot 38$ |
| PS $\rightleftharpoons$ | $T+Q+B-$ | $0 \cdot 001731$ | $19 \cdot 19$ | $759 \cdot 31$ |
| PS $\rightleftharpoons$ | $T+Q+C-$ | $0 \cdot 001621$ | $18 \cdot 83$ | $754 \cdot 38$ |
| PS $\rightleftharpoons$ | $T+Q+D-$ | $0 \cdot 000774$ | $19 \cdot 63$ | $764 \cdot 43$ |

| | Mean | gr. | | |
|---|---|---|---|---|
| PS $\rightleftharpoons$ | $T+Q+\frac{1}{4}(A+B+C+D)-$ | $0 \cdot 00177$ | $19 \cdot 28$ | $759 \cdot 12$ |

whence, supposing U to have the same density as V,

$PS=7000 \cdot 00090$ grains, of which U contained 5760.

Results of comparisons of PC No. 1, PC No. 2, PC No. 3, PC No. 4 with PS :—

| | gr. | No. of Comparisons. |
|---|---|---|
| PC No. $1=PS+$ | $0 \cdot 00051$ | 200 |
| PC No. $2=PS-$ | $0 \cdot 00089$ | 216 |
| PC No. $3=PS-$ | $0 \cdot 00178$ | 204 |
| PC No. $4=PS-$ | $0 \cdot 00316$ | 204 |

The weights Sp, Sb, K were returned to Professor Schumacher accompanied by a weight V such that, by a mean of 200 comparisons, $Sp+V \rightleftharpoons PS-0 \cdot 00071$ grain in air $(t=13 \cdot 1, \ b=759 \cdot 09)$.

By the good offices of M. Arago, permission was obtained from the French Government to compare the new English weights with the standard kilogramme of platinum, known as the *kilogramme des Archives*, and which will be denoted by the letter 𝔄. The comparison was made by two perfectly independent methods. In one of these 𝔄 was compared sixty times with PC No. 1 + PC No. 2 + auxiliary weight B + a platinum weight V of nearly 192·436 grains. In the other, 𝔄 was first compared 200 times with the platinum kilogramme 𝔈, purchased for the British Government. 𝔈 was afterwards compared with PS + each of the four platinum copies of the pound in succession, together with a platinum weight of about 1432·324 grains, the weight of which was found with great precision by a process to be described presently.

𝔄 had never been weighed in water. By observations made with the stereometer, it was found that at 0° C. the volume of 𝔄 exceeded that of 𝔈 by a quantity equal to the volume of 21·119 grains of water at its maximum density. By weighing 𝔈 in air and in water, it was found that $\Delta\mathfrak{E} = 20\cdot54877$. Some time after these observations were made, the Committee received from Professor Schumacher some observations of his own in manuscript, and a copy of Professor Steinheil's paper, entiled 'Das Bergkrystall-kilogramm,' from the fourth volume of the Transactions of the Bavarian Academy of Sciences, containing the determination of the volume of 𝔄, by comparing its linear dimensions with those of a platinum kilogramme of his own 𝔖, the density of which had been found by weighing it in air and in water. The two weights being cylinders, and the linear dimensions measured with an extremely delicate instrument constructed by Gambey, this kind of observation admitted of being made with great accuracy. The resulting difference between the volume of 𝔄 and that of 𝔈, was found to be equal to the volume of 20·933 grains of water at its maximum density. On account of the large number of observations, and the extreme care with which they were made, this value of the volume 𝔄 — volume 𝔈 is to be preferred to that which was obtained by the stereometer, and has accordingly been used in reducing the observations for comparing the weights of 𝔄 and 𝔈.

𝔈 was compared with PS by the method which had proved so satisfactory in deducing the avoirdupois pound from the troy pound. Let I, K, L, M, N denote PS and its four platinum copies, A, B, Γ, Δ, platinum weights of about 1432·322 grains each, Z a weight of about 1270·708 grains, Θ a weight of 161·629 grains, made up of weights the values of which had been carefully determined. 𝔈 was compared with each of the weights I + K + A, I + L + B, I + M + Γ, I + N + Δ, each of the lbs. K, L, M, N having been previously compared with I ; I with A + B + Γ + Δ + Z ; each of the weights A, B, Γ, Δ with Z + Θ. In this manner it was found that the kilogramme des Archives weighed 15432·34874 grains, of which the new Imperial Standard pound contains 7000, or kilogramme = 2·20462125 lbs. This is probably the best determination of the weight of 𝔄 in terms of the English standard of weight.

The value of 𝔄, as deduced from the direct comparison of 𝔄 with

$K + L + B + V$, is subject to some uncertainty, arising from the circumstance that the platinum, of which A, B, C, F were made, had been very badly prepared and contained cavities filled with some hygroscopic substance which rendered the weight of B slightly variable, according to the greater or less amount of moisture present in the atmosphere. According to these observations, the kilogramme des Archives $= 15432 \cdot 34816$ grains.

By the observations of Schumacher and Steinheil on the ratio of the weight of 𝔄 to that of Sp, subject to an uncertainty of $0 \cdot 00139$ grain, on account of an error of the press, and the comparison of Sp with PS, the weight of 𝔄 is either $15432 \cdot 34873$ or $15432 \cdot 35012$ grains, of which PS contains 7000.

The French standard of commercial weight is a brass kilogramme 𝔏, known as the *kilogramme type laiton*. It is deposited at the Ministère de l'Intérieure. According to a comparison of 𝔏 with 𝔄, the result of which is published in the 25th volume of the Modena Transactions, the apparent weight of 𝔏, when weighed in air at Somerset House, the mercury in the barometer, reduced to the freezing-point, standing at $29 \cdot 75$ inches, and the thermometer at $65 \cdot 66$ F. ($b = 755 \cdot 64$ mm., $t = 18 \cdot 7$ C.), is $15432 \cdot 344$ grains, of which the English commercial standard contains 7000.

---

## LXXI. *Intelligence and Miscellaneous Articles.*

### NOTE ON THE NEW ZEOLITE FROM SKYE, ANALYSED BY MALLET. BY DR. HEDDLE.

THE mineral analysed by Mallet (Silliman's American Journal, Sept. 1856, p. 179) is Laumonite, somewhat effloresced from having been " several years " in his possession. An analysis of a similar specimen was published by Scott (Jameson's Journal, October 1852, vol. liii. p. 284), which specimen also bore the name hypostilbite, and I have seen many other specimens in cabinets in this country with the same name attached. As it bears but very slight resemblance to the substance called hypostilbite by Beudant, the christening may be explained by the ignorance of some unprincipled dealer resorting to a rare title in order to rid himself of a superabundance of an insignificant-looking substance. It occurs in a vein at the Storr in Skye, in immense quantity; the vein has an average thickness of about $1\frac{1}{2}$ inch, and the appearance is exactly as described,—" a mass of minute crystals resembling loaf-sugar, breaking easily, and crumbling under the fingers into a coarsish crystalline powder." The external characters of hardness, gravity, transparency, &c., as given, agree with those of Laumonite;—the latter has a pearly lustre on cleavage planes, and is monoclinic; gelatinizes, which stilbitic minerals do not do; and the fact that minute crystals of stilbite do occur in small cavities of the substance, may explain the $\cdot 87$ of potash with some soda. Being Laumonite, the several years have told upon the proportion of water, but in other respects the analysis agrees with those of Laumonite, especially with that of Scott; and it may be observed that those by Connell and Babo, of Skye speci-

mens, differ from the per-centage proportions required by the formula in precisely the same particulars.

I heartily agree with Mr. Mallet in the opinion he expresses in his concluding sentence, that it is far from "desirable to add to the already numerous names of stilbite-like minerals;" and the sincerity of his expressed opinion is most strikingly evinced by his refraining from naming the substance till time should have tested its specific individuality.

P.S. For the sake of comparison with Mr. Mallet's description, I quote from a paper (which I have just laid my hands upon) on the Geology and Mineralogy of Skye, read some years ago at the Edinburgh Geological Society. Speaking of the minerals to be found at the Storr, "Laumonite is here found in veins of about 1 inch in thickness, which consist of a congeries of minute crystals, so confused as to have a granular appearance; this mineral, strange to say, used to pass under the name of hypostilbite, and under that name came into Dr. Scott's hands, who analysed it."

## ON RIESS'S LAW OF ELECTRICAL HEAT.

### *To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I believe the great question at issue between M. Riess and myself is the exactitude of a law of "electrical heat" which M. Riess thinks he has discovered. Now, without seeking to prolong a painful controversy, or further canvas the accuracy of the several statements relative to myself found in M. Riess's last communication, I trust you will be so good as to grant me the privilege of a few lines immediately bearing upon the law of electrical heat just referred to.

The course of experimental research pursued by M. Riess amounts to this:—He accumulates and discharges through a wire a given quantity of electricity. First the given quantity is collected on a certain number of coated jars, then on a larger number. He says, that in the latter case the tension or intensity (or whatever he may please to call it) of the accumulated charge has suffered a change, and that in consequence of this change the heating power is diminished; that the heat is in fact inversely proportional to the surface over which the electricity has been expanded, all other things being the same.

I take occasion, in opposition to all this, to remind M. Riess of some well-known phænomena of electrometer indications, and certain generally admitted laws of electrical force and measurement by such instruments. I show, and I believe very clearly, that these instruments signalize nothing whatever of any change in *quality* of the accumulated charge; that they are altogether influenced by conditions quite foreign to all such views; that what we learn from

such instruments is merely the relative state of activity of the whole or of a portion of the charge, in a given direction, taken in terms of a given statical force, either attractive or repulsive, and by the instrumentality of which we may occasionally infer the *quantity* of electricity in operation at the instant of discharge. I further show, by reference to investigations prior to those of Dr. Riess, that the diminished effect of the electrical discharge observed in accumulating and discharging a given quantity of electricity from an increased surface, taken in parts, as in the case of extending a battery by the addition of other jars, arises solely from the resistance we thereby introduce into the circuit of discharge, and not from any change of tension in the accumulated electricity itself; and in confirmation of this, I call Dr. Riess's attention to the fact, as a crucial experiment, that when the given quantity of electricity is accumulated on an *undivided surface*, that is to say, when it is collected on the extended surface of a single large jar, for example, instead of an extension of surface by means of several lesser jars, the surfaces being in each case alike extended,—or when collected on two separate jars of such unequal surfaces and magnitudes as to give electrometer indications of extremely different intensities,—then no change takes place in the heating effect of the discharge; consequently the mere extension of the surface of the battery, taken alone, has really no influence on the result; and Dr. Riess's law of electrical heat falls to the ground. Now to this experiment Dr. Riess makes no reply.

It would be easy for me to animadvert effectively upon all Dr. Riess has advanced in his last communication. I am not certainly open to the censure of having taken Haldane's method of measuring quantity as my own; and in confirmation of this I beg to refer to my paper in the Transactions of the Plymouth Institution, also to page 358, Phil. Mag. vol. xi., referred to by Dr. Riess himself. I certainly resorted to the method at first, without being aware that the same method had been already employed by Haldane; but I was very cautious in claiming any precedence, or stating, as Dr. Riess has done (Pogg. *Ann.* vol. xl. p. 324), that I was the " first to apply this method" practically, although I certainly employed it many years previously to the appearance of Dr. Riess's paper in Poggendorff, and precisely in the way described by him, that is to say with a Lane's bottle in communication with the external coating, instead of a Cuthbertson's balance as used by Haldane.

I have the honour to remain, Gentlemen,

Your very obliged and obedient Servant,

6 Windsor Villas, Plymouth,                     W. Snow Harris.
   Dec. 20, 1856.

# INDEX to VOL. XII.

END OF THE TWELFTH VOLUME.

2